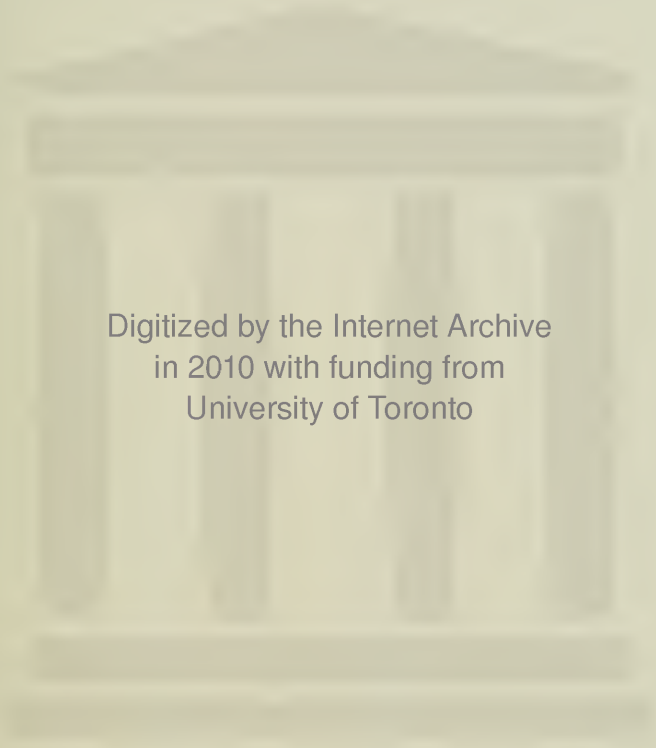

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Michl Longridge

PRESIDENT.

THE INSTITUTION

OF

MECHANICAL ENGINEERS.

ESTABLISHED 1847

PROCEEDINGS.

1917, v.1

JANUARY–MAY.

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The Institution of Mechanical Engineers.

PAST-PRESIDENTS.

George Stephenson, 1847–48. (*Deceased* 1848.)
Robert Stephenson, F.R.S., 1849–53. (*Deceased* 1859.)
Sir William Fairbairn, Bart., LL.D., F.R.S., 1854–55. (*Deceased* 1874.)
Sir Joseph Whitworth, Bart., D.C.L., LL.D., F.R.S., 1856–57, 1866. (*Deceased* 1887.)
John Penn, F.R.S., 1858–59, 1867–68. (*Deceased* 1878.)
James Kennedy, 1860. (*Deceased* 1886.)
The Right Hon. Lord Armstrong, C.B., D.C.L., LL.D., F.R.S., 1861–62, 1869. (*Deceased* 1900.)
Robert Napier, 1863–65. (*Deceased* 1876.)
John Ramsbottom, 1870–71. (*Deceased* 1897.)
Sir William Siemens, D.C.L., LL.D., F.R.S., 1872–73. (*Deceased* 1883.)
Sir Frederick J. Bramwell, Bart., D.C.L., LL.D., F.R.S., 1874–75. (*Deceased* 1903.)
Thomas Hawksley, F.R.S., 1876–77. (*Deceased* 1893.)
John Robinson, 1878–79. (*Deceased* 1902.)
Edward A. Cowper, 1880–81. (*Deceased* 1893.)
Percy G. B. Westmacott, 1882–83.
Sir Lowthian Bell, Bart., LL.D., F.R.S., 1884. (*Deceased* 1904.)
Jeremiah Head, 1885–86. (*Deceased* 1899.)
Sir Edward H. Carbutt, Bart., 1887–88. (*Deceased* 1905.)
Charles Cochrane, 1889. (*Deceased* 1898.)
Joseph Tomlinson, 1890–91. (*Deceased* 1894.)
Sir William Anderson, K.C.B., D.C.L., F.R.S., 1892–93. (*Deceased* 1898.)
Sir Alexander B. W. Kennedy, LL.D., F.R.S., 1894–95.
E. Windsor Richards, 1896–97.
Samuel Waite Johnson, 1898. (*Deceased* 1912.)
Sir William H. White, K.C.B., LL.D., D.Sc., F.R.S., 1899–1900. (*Deceased* 1913.)
William H. Maw, LL.D., 1901–02.
J. Hartley Wicksteed, 1903–04.
Edward P. Martin, 1905–06. (*Deceased* 1910.)
T. Hurry Riches, 1907–08. (*Deceased* 1911.)
Sir John A. F. Aspinall, M.Eng., 1909–10.
Edward B. Ellington, 1911–12. (*Deceased* 1914.)
Sir H. Frederick Donaldson K.C.B., 1913–14. (*Deceased* 1916.)
W. Cawthorne Unwin, LL.D., F.R.S., 1915–16.

The Institution of Mechanical Engineers.

OFFICERS.

1917.

PRESIDENT.

MICHAEL LONGRIDGE, ... Altrincham.

PAST-PRESIDENTS.

SIR JOHN A. F. ASPINALL, M.Eng., ... Manchester.
SIR ALEXANDER B. W. KENNEDY, LL.D., F.R.S., ... London.
WILLIAM H. MAW, LL.D., ... London.
E. WINDSOR RICHARDS, ... Caerleon.
W. CAWTHORNE UNWIN, LL.D., F.R.S., ... London.
PERCY G. B. WESTMACOTT, ... Ascot.
J. HARTLEY WICKSTEED, ... Leeds.

VICE-PRESIDENTS.

WILLIAM H. ALLEN, ... Bedford.
DUGALD CLERK, D.Sc., F.R.S., ... London.
HENRY DAVEY, ... Ewell.
SIR ROBERT A. HADFIELD, BART., D.Sc., D.Met., F.R.S., Sheffield.
J. ROSSITER HOYLE, ... Sheffield.
MARK H. ROBINSON, ... London.

MEMBERS OF COUNCIL.

ARCHIBALD BARR, LL.D., D.Sc., ... Glasgow.
SIR J. WOLFE BARRY, K.C.B., LL.D., F.R.S., ... London.
SIR A. TREVOR DAWSON, ... London.
JOHN DEWRANCE, ... London.
SIR J. ALFRED EWING, K.C.B., LL.D., F.R.S., ... Edinburgh.
CHARLES HAWKSLEY, ... London.
H. S. HELE-SHAW, LL.D., D.Sc., F.R.S., ... London.
GEORGE HUGHES, ... Horwich.
HENRY A. IVATT, ... Haywards Heath.
BOWMAN MALCOLM, ... Belfast.
ROBERT MATTHEWS, ... Manchester.
DONALD B. MORISON, ... Hartlepool.
SIR GERARD A. MUNTZ, BART., ... Birmingham.
ENG. VICE-ADMIRAL SIR HENRY J. ORAM, K.C.B., F.R.S., .. London.
WILLIAM H. PATCHELL, ... London.
WALTER PITT, ... Bath.
SIR VINCENT L. RAVEN, ... Woolwich.
WILLIAM REAVELL, ... Ipswich.
CAPTAIN H. RIALL SANKEY, C.B., R.E., ret., ... London.
RICHARD WILLIAMSON, ... Workington.
WILSON WORSDELL, ... Ascot.

HON. TREASURER.
F. H. NORWOOD.

AUDITOR.
RAYMOND CRANE, F.C.A.

SECRETARY.
EDGAR WORTHINGTON,

The Institution of Mechanical Engineers,
Storey's Gate, St. James's Park, London, S.W., 1.
Telegraphic Address:—*Mech, Parl, London.* Telephone:—*Victoria*, 4564.

The Institution of Mechanical Engineers.

PROCEEDINGS.

ANNUAL REPORT OF THE COUNCIL FOR THE YEAR 1916.

The Council have pleasure in presenting to the members at the Seventieth Annual General Meeting their Report of the progress and work of the Institution during the year.

The changes which have taken place in the roll during 1916 are shown in the subjoined tabulated statement:—

	Hon. M.	M.	A.M.	A.	G.	Totals.	
Totals at 31st December 1915	6	2,717	3,026	50	520		6,319
Elected	3	61	164	1	33		
Reinstated	—	2	2	—	1		
Transferred	—	44	—	—	—		
Total additions during 1916	3	107	166	1	34	311	
Deceased	2	43	22	3	10		
Resigned	—	28	27	2	4		
Erased	—	27	65	—	18		
Graduates elected Associate Members	—	—	—	—	40		
Graduates retired under By-law 3.	—	—	—	—	42		
Elections voided	—	—	3	—	—		
Transferred	—	3	44	—	—		
Total deductions during 1916	2	101	161	5	114	383	
Net alterations during 1916	+1	+6	+5	−4	−80		−72
Totals at 31st December 1916	7	2,723	3,031	46	440		6,247

His Majesty the King has conferred the following honours upon members :—

Baronetcy :—Mr. Alfred F. Yarrow.

Companionship of the Order of the Bath :—Eng. Commr. G. E. Andrew, R.N.; Eng. Capt. D. P. Green, R.N.; Capt. H. Riall Sankey, R.E.

Companionship of the Order of the Star of India :—The Right Hon. Lord Montagu of Beaulieu.

Membership of the Royal Victorian Order :—Eng. Lieut.-Commr. R. C. Hugill, R.N.

Distinguished Service Order :—Lieut.-Col. C. G. Bradley, King's L'pool Regt.; Capt. A. E. Davidson, R.E.; Capt. H. M. Leaf, R.M.L.I.; Eng. Commr. R. W. Skelton, R.N.; Capt. H. C. Sparks, London Scottish; Major S. J. Thompson, R.F.A.

Military Cross :—Capt. S. Bramley-Moore, A.S.C.; Capt. A. O. Laird, R.E.; Lieut. H. S. Manisty, R.E. (S.R.); Capt. R. B. Pitt, R.E. (T.); Lieut. W. G. Scotcher, E. Yorks. Regt.; Trooper A. W. Wingate, 1st Royal Dragoons.

Distinguished Service Cross :—Capt. E. H. Lamb, R.N.V.R.; Lieut. E. R. Peal, R.N.V.R.

Albert Medal :—Lieut. H. J. Higgs, R.E. (S.R.).

Mr. E. D. Leavitt, Mr. Richard Price-Williams, and Sir Alfred F. Yarrow, Bart., were nominated by the Council as Honorary Life Members of the Institution, on account of their eminent scientific attainments and their distinction as Engineers.

Deceases of the following members (with dates of election) were reported during the year. The names of those who lost their lives in connexion with the War are printed in heavier type :—

1913. ARUNACHELAYER, T. K. (*A.M.*)
1892. AUSTIN, J. MEREDITH
1891. BEATTY, HAZLITT M., C.M.G.
1890. BLACKBURN, JOHN
1898. BLACKSTONE, EDWARD C.
1910. BRECKNELL, HENRY
*1914. **BRUCE, A. C. A.** (*G.*)
*1914. **BUMPUS, BERNARD E.** (*A.M.*)
1910. BUTTERS, HOWARD
1903. CHRISTIANSEN, A. G., JUN. (*A.M.*)

* Killed in action or died of wounds.

*1912. **CORFE, ALLAN WILLIAM** (*G.*)
1900. CROSTA, LORENZO WILLIAM
*1903. **DAVIDSON, JAMES SAMUEL**
1903. **DEAKIN, GEORGE WELSBY**
1899. DOBSON, SYDNEY THORNTON
1898. **DONALDSON, Sir H. FREDERICK, K.C.B.** (*Past-President*)
*1912. **DRUITT, CHARLES L.** (*G.*)
1914. DURHAM, HARRY WILLIAM
1888. EATON-SHORE, GEORGE
1907. EDGCOMBE, WILLIAM EDWARD (*A.M.*) (*Deceased* 1915)
1914. EDWARDS, WILLIAM GEORGE (*A.M.*) (*Deceased* 1915)
1898. ELLIS, JESSE
1909. FULLER, GEORGE E. (*A.M.*)
1899. GARRARD, GEORGE MINGAY
1901. GAWTHORNE, WILLIAM A.
*1910. **GORDON, VIVIAN** (*A.M.*) (*Deceased* 1915)
1900. GRANT, LEWIS
1895. GROVES, MONTAGUE (*A.M.*)
1870. GWYNNE, JAMES EGLINTON ANDERSON (*Deceased* 1915)
1875. HAMMOND, WALTER JOHN
1901. HANNING, WILLIAM
*1913. **HARRIS, LYN ARTHUR P.** (*G.*)
*1914. **HOWARTH, JOHN** (*A.M.*)
1888. INGLEBY, JOSEPH
1888. JAQUES, Capt. W. H., U.S.N.
1902. JEFFREYS, JOHN
1905. JOHNSON, FREDERICK DWIGHT (*Deceased* 1915)
*1912. **JOHNSTON, A. D., JUN.** (*A.M.*)
*1913. **KEYMS, THOMAS BOOTH**
1872. KING, WILLIAM
1883. LAYCOCK, WILLIAM S.
1883. LEAVITT, ERASMUS DARWIN (*Hon. Life Member*)
1890. LEE, SAMUEL EDWARD
1900. LUPTON, WILLIAM WALTER
1907. MACFARLANE, JAMES B. (*A.M.*)
1904. MCLEAN, ALEXANDER MALKER
1884. MAIS, HENRY COATHUPE
1884. MAXIM, Sir HIRAM STEVENS
1903. MAY, CHARLES RAMSDEN
1891. MEADE, THOMAS DE COURCY
1898. MICKLEWRIGHT, WILLIAM
1912. MINETT, A. E. S. (*A.M.*)
1910. **NORTHCOTT, H. H. M.** (*A.M.*)
*1912. **OKEY, WILLIAM EWART** (*G.*)
*1913. **POYNTING, ARTHUR** (*A.M.*)
1867. PRATCHITT, JOHN
1859. PRICE-WILLIAMS, RICHARD (*Hon. Life Member*)
*1914. **REYNOLDS, WILLIAM A.** (*A.M.*)
1892. **ROBERTSON, LESLIE STEPHEN**
*1914. **ROBERTSON, ROBERT S.** (*G.*)
*1907. **ST. LEGIER, Eng. Lt.-Commr. ARCHIBALD S. DE, R.N.** (*A.M.*)
1876. SHIELD, HENRY
1905. SMITH, MATTHEW SIDNEY (*A.M.*) (*Deceased* 1915)
1881. SMITH, Professor ROBERT. H.
*1914. **STEPHENS, HUGH G.** (*G.*)
1905. VICKERS, ERNEST JOHN (*A.M.*) (*Deceased* 1915)
*1911. **VYALL, LIONEL EGERTON** (*G.*)
1909. WALKER, JAMES
1897. WATSON, THOMAS JOHN
1897. WAYNFORTH, Professor H. M.
*1911. **WELSH, ANTHONY R.** (*A.M.*)
*1913. **WILKINS, FRANK TREVOR** (*G.*)
1873. WILSON, THOMAS SIPLING
1881. WOOD, EDWARD MALCOLM
1882. WOODALL, Sir CORBET, D.Sc.

* Killed in action or died of wounds.

Of these, Sir Frederick Donaldson was a Member of Council from 1905, a Vice-President 1910–12, and President in 1913 and 1914; and Mr. Price-Williams, elected in 1859, was a Member of Council 1880–1887.

In connexion with the loss of Sir Frederick Donaldson and Mr. Leslie Robertson, while proceeding to Russia with Lord Kitchener in H.M.S. "Hampshire," as Members of his Staff, a Memorial Service, arranged at the request of the Presidents of the five leading British Engineering Institutions, was held at St. Margaret's Church, Westminster, on 15th June. The decease of Sir Frederick is a very great misfortune, not only to the Institution, in whose activities he took so great an interest, and whose welfare he had so much at heart, but also to the Nation. He had occupied with distinction the position of Chief Superintendent of the Ordnance Factories at Woolwich for many years, and shortly before his death became Chief Technical Adviser to the Ministry of Munitions of War. Mr. Leslie Robertson's death is also to be deplored, in view of the important work he carried out in connexion with the Engineering Standards Committee.

The Accounts for the year ended 31st December 1916 are submitted (*see* pages 16 to 21), having been duly certified by the Auditor. The total revenue was £22,444 1*s.* 8*d.*, while the expenditure was £12,628 18*s.* 11*d.*, leaving a balance of revenue over expenditure of £9,815 2*s.* 9*d.* The total investments and other assets amount to £137,588 10*s.* 7*d.*, and, deducting therefrom the £34,025 of debentures, a temporary loan of £6,000, and other liabilities, including the sum set aside for the Leasehold Redemption Fund, the capital of the Institution amounts to £75,691 12*s.* 6*d.* The loan from Messrs. R. and R. H. Williamson has been reduced by another £1,000; and the total Leasehold Redemption Fund has been increased to £19,083 8*s.* 11*d.* The certificates of all securities have been duly verified by the Finance Committee and the Auditor.

Notwithstanding the War, the greater part of the work of the

Institution has been carried on, but the usual Summer Meeting, Anniversary Dinner, and Conversazione were not held. The General Meetings in London have been held at The Institution of Civil Engineers, by kind permission of their Council, to whom also the members are indebted for Reading Room and Library facilities. The names of over 900 members who are, or have been, on Active Service during the year, are printed in Part I of the Journal. Additional names have been received. Many other members are engaged on munition work. Six members of the Institution Staff are now on Active Service, and two others have received temporary postponement. Their places have been partly filled by women clerks.

The practice of keeping at the Institution brief statements of the professional qualifications of members desiring to undertake work in connexion with the War has been continued during the year, and copies have been forwarded to the Ministry of Munitions of War and other Government Departments.

Early in the year, the Council had under consideration some cases of members unable to continue their Subscriptions owing to circumstances arising out of the War. To meet this hardship, two new By-laws were framed giving power to the Council, under certain circumstances, to remit the Subscription of any member on Active Service or other National War Service, or to remit the Entrance Fee on re-election of any member whose membership had ceased on account of financial difficulty due to the War. These By-laws were duly passed by the members at the Annual General Meeting in February (Proceedings 1916, pages 48 and 136).

The marble memorial of the late Sir William H. White, K.C.B., was unveiled in the Entrance Hall of The Institution of Civil Engineers on 26th July. The Sir William White Research Scholarship in Naval Architecture (of the annual value of £100) is being administered by The Institution of Naval Architects and will be thrown open to competition after the War.

The Thomas Hawksley Gold Medal for 1917, given annually for the best Paper published in the Proceedings of the preceding year, has been awarded to Mr. Daniel Adamson, *Member*, for his Paper on "Spur-Gearing" (Proceedings 1916, page 353); and a Premium of £10 has been awarded to Sec. Lieut. Robert W. Fenning, R.E. (T.), B.Sc., D.I.C., for his Paper on "The Composition of the Exhaust from Liquid-Fuel Engines" (Proceedings 1916, page 185).

The third Award of the Starley Premium, for the best Paper published in the Proceedings of 1914, 1915, and 1916 dealing with the Development of Road Locomotion, has been made to Mr. Robert E. Phillips, *Member*, for his Paper on "Variable-Speed Gears for Motor Road-Vehicles" (Proceedings 1916, page 783). The fourth Award will be made in February 1920.

The fifth Award of the Water Arbitration Prize, for the best Paper published in the Proceedings of 1915 and 1916 dealing with certain specified subjects connected with Water, has been made to Mr. Walter Clemence, *Member*, for his Paper on "Theory and Practice in the Filtration of Water" (Proceedings 1916, page 239). The special Regulations for the sixth Award in February 1919 are printed upon page 22.

A Declaration of Trust for the Bryan Donkin Fund (pages 22–25) has been prepared and sealed by the Institution. In Clause 4 it will be seen that the income of the Fund is to be devoted to grants in aid of original research in the science and practice of Mechanical Engineering. Mr. A. H. Barker, the recipient of a Grant from the Fund in February, has procured the apparatus for his Research "to investigate a new method of determining the radiant temperature and air temperature in a room." The experiments are being conducted at University College, London, and the results will be embodied in a Paper to be submitted to the Institution. The next Award will be made in February 1919.

The Institution Examinations were held in October. Ten Candidates passed the Graduateship and eight the Associate

Membership Examination. On the results of the Associate Membership Examination, a Prize of the value of £5 was awarded to L. H. Thomas. The Examination Papers have been printed in pamphlet form. The next Examinations will be held in October 1917.

In January a Conference of representatives of the five leading British Engineering Institutions was held at The Institution of Civil Engineers, to consider generally the Government Scheme (23rd July 1915) for the Organization and Development of Scientific and Industrial Research, and the Constitution of the Advisory Council. On the recommendation of the Conference, a Petition was presented to H.M. Privy Council, urging that the Advisory Council should be strengthened by the addition of some representatives of Engineering industries. The Petition having been declined, a further Conference was held, at which a Deputation was appointed to wait upon the Lord President of the Privy Council, Lord Crewe, and put forward some considerations as to the representation of Engineering on the Advisory Council. The Deputation was courteously received by the Lord President, who explained that the Privy Council did not at that time see their way to modify the constitution of the Advisory Council. [Subsequently it was officially announced that a Charter had been granted to "The Imperial Trust for the Encouragement of Scientific and Industrial Research" (hereinafter referred to as the Government Department of Research).] Dr. Dugald Clerk, F.R.S., *Vice-President*, was appointed to represent the Institution on the Engineering Committee of the Government Department of Research. Grants for one year have been made in aid of three Researches, carried out under the direction of the Institution, namely:—Alloys, £200; Steam-Nozzles, £300; and Hardness Tests, £100.

In view of the work now being undertaken by the Government Department of Research, it has been considered desirable to take no further steps with regard to the proposal to form a General

Engineering Research Committee for the collection and co-ordination of Engineering Research. The information obtained since the Conference, held at the Institution in February 1914, with representatives of Universities, Scientific Societies and large British Railways and Engineering Firms, has been placed at the disposal of the Government Department.

Investigations into the alloys of aluminium with zinc and copper, under the direction of the Alloys Research Committee, have been continued at the National Physical Laboratory, but the publication of the Eleventh Report has been necessarily deferred until after the War. A grant of £200 towards the prosecution of the research during the year 1916 was received from the Government Department of Research, and this sum, with a like amount from the Institution funds, has been handed over to the National Physical Laboratory. The Committee has been enlarged by the appointment of Mr. John Dewrance, *Member of Council*, as Chairman, in place of the late Sir William White; Sir Gerard A. Muntz, Bart., *Member of Council;* and Mr. William Mills, *Member.*

The Hardness Tests Research Committee, under the Chairmanship of Dr. W. Cawthorne Unwin, F.R.S., have carried on their investigations at the National Physical Laboratory, with the object of measuring the resistance to wear on rolling or sliding surfaces and comparing it with indentation hardness. A testing-machine of a new type was designed by Dr. T. E. Stanton, F.R.S., and a number of specimens of steel were tested, including samples kindly supplied by Sir Robert Hadfield. The First Report of the Committee (Proceedings 1916, page 677) contained the results of experiments made by Dr. Stanton and Mr. R. G. Batson, *Associate Member*, together with Appendixes by Dr. Unwin, Sir Robert Hadfield, Dr. A. E. H. Tutton, and Professor T. Turner. The Government Department of Research granted a sum of £100 for this work, and a grant of £10 was made by the Council as part of the Third Award of the Bryan

Donkin Fund. During the discussion of the Report, Sir Robert Hadfield expressed his desire to offer, through the Council, a Prize for a new and accurate method of determining hardness of metals. The offer has been gratefully accepted.

The Steam-Nozzles Research Committee have still under consideration the design of apparatus for conducting experiments relating to the Action of Steam passing through Nozzles and Steam-Turbines. Some experiments, using jets of compressed air in a cage, designed by Mr. H. M. Martin, are referred to in the discussion of Captain T. B. Morley's Paper (Proceedings 1916, page 89), and some further trials with this cage have been made. As already stated, a grant of £300 has been received from the Government Department of Research towards this work, and £10 has been granted by the Council from the Bryan Donkin Fund. Captain H. Riall Sankey, C.B., R.E., *Member of Council*, has continued to act as Chairman ; and Professor A. L. Mellanby, D.Sc., *Member*, and Professor J. E. Petavel, F.R.S., *Member*, have been appointed Members of the Committee.

Although the work of the Wire Ropes Research Committee has been delayed on account of the War, further proposals for the testing-machine have been under consideration, and alternative modified designs are now in preparation.

Mr. Richard Williamson, *Member*, has offered to the Government Department of Research £500 towards a Mechanical Engineering Research, to be conducted by the Institution. The best way of utilising this generous gift has received consideration, and the members have been invited to make suggestions for such a research.

With a view to assisting the maintenance and extension of British Engineering Trade after the War, the British Standards Committee have determined to translate their Specifications into French, Spanish, and Russian, to give metric equivalents of English measures, and to issue them at a much lower price than has

hitherto been possible. Arrangements are being made to establish local Committees in overseas and foreign countries, to obtain information and facilitate commercial transactions. The cost will be large, and an appeal was made to the Institution for a contribution to the fund which is being raised. The matter is considered so urgent that the work is already in hand. It was resolved to grant a sum of £500 as a donation to the fund, the Council feeling that this work is of vital importance to British Engineering Firms.

In response to an invitation from the Marine Department of the Board of Trade, for observations upon a suggested amendment of the Board of Trade Rule for Nominal Horse-Power of Marine Engines, the Council, while not concurring in the adoption of the new Rule, offered to appoint a Committee to consider the formulation of a horse-power unit based on engineering principles. The Board of Trade, however, considered that the present was not an opportune time for such an investigation, and ultimately decided to adopt their suggested new Rule, with a slight modification.

The Council have been glad to give their support to a Report on the Education of the Marine Engineer, drawn up by a Committee of the Liverpool Engineering Society. The efforts of this Committee were directed to raising the standard of the Marine Engineer, both as regards theory and practice. The Report is being submitted to the Board of Trade.

The following Advisory Committees have been duly constituted by the Council to give advice and to make representations on matters affecting the members in India and South Africa respectively :—

Indian Advisory Committee.

The Chairman of the Calcutta and District Section, *ex officio.*
Mr. W. E. Buchanan, Simla.
The Hon. Sir Francis J. E. Spring, K.C.I.E., Madras.
Major G. H. Willis, M.V.O., R.E., Bombay, *Corresponding Member.*

South African Advisory Committee.

Mr. John Harbottle, Kimberley.
Mr. Arthur MacNay, Durban.
Professor John Orr, Johannesburg, *Corresponding Member.*
Mr. Theodore Reunert, Johannesburg.
Mr. J. A. Vaughan, Johannesburg.

Monthly General Meetings were held throughout the year, with the exception of June, July, August and September. These Meetings were occupied with the reading and discussion of the following Papers:—

The Flow of Air through Nozzles; by Captain Thomas B. Morley, B.Sc., *Associate Member.*

Chisels; by Henry Fowler, *Member.*

The Composition of the Exhaust from Liquid-Fuel Engines; by 2nd Lieut. Robert W. Fenning, R.E. (T.), B.Sc., D.I.C.

Theory and Practice in the Filtration of Water; by Walter Clemence, *Member.*

Spur-Gearing; by Daniel Adamson, *Member.*

Trials on a Diesel Engine, and Application of Energy Diagram to obtain Heat Balance; by the late Lieut. F. Trevor Wilkins, M.Sc., *Graduate.*

Report of the Hardness Tests Research Committee.

Variable-Speed Gears for Motor Road-Vehicles; by Robert E. Phillips, *Member.*

The following Paper was accepted for publication in the Proceedings, with discussion in writing:—

Modules and Uniform Discharge Devices for Irrigation and Waterworks; by Hugh Munro, *Member.*

The following Papers were selected for publication in the Proceedings:—

Propelling Machinery for Ships; by W. J. Drummond, *Graduate.*

Small Machinery in the Argentine Republic; by F. A. Moffatt, *Associate Member.*

Mr. Harry E. Jones, *Member*, delivered the fourth "Thomas Hawksley" Lecture, on "The Gas Engineer of the Last Century" (Proceedings 1916, page 631), in London on 3rd November. The Lecture was repeated by Mr. Jones in Cardiff on 9th November.

Captain H. Riall Sankey, C.B., R.E., *Member of Council*, has accepted an invitation to deliver the "Thomas Hawksley" Lecture 1917.

The thanks of the Institution have been expressed to the following gentlemen who kindly acted as Honorary Local Correspondents, to make arrangements for Local Meetings, during the Session 1915–16:—

Mr. John W. Hall, *Member* (Birmingham).
Mr. A. Matthews (Manchester).
Mr. David E. Roberts, *Member* (Cardiff).
Mr. E. W. Fraser Smith (Newcastle-on-Tyne).

Two Local Meetings were held in Cardiff and one each in Manchester and Sheffield, for the further discussion of Papers presented in London and for the repetition of the "Thomas Hawksley" Lecture. Invitations were extended to the Members of the Local Engineering and Metallurgical Societies to attend and take part in these Meetings.

The Council desire to thank the members and others who have made presentations to the Library. A complete list of additions will be found on pages 26–40. During the year more than 200 works have been lent to members, and about 100 searches for special information have been made.

The Institution has been represented on the—

Courts of the Universities of Bristol, Liverpool, and Sheffield.
Governing Board of the Imperial College of Science and Technology.
General Committee of the Royal Society for administering the Government Grant for Scientific Investigations.
National Physical Laboratory.
Engineering Committee of the Government Department of Research.
Engineering Standards Main Committee and Sectional Committees.

School of Metalliferous Mining (Cornwall).
London Committee of the University of Hong Kong.
Board of Scientific Societies, organized by the Royal Society.
Steel Research Committee of The Institution of Automobile Engineers and The Society of Motor Manufacturers and Traders.
Corrosion Committee of The Institute of Metals.
Conference at the Board of Education to consider a scheme for recording the educational work of interned British Prisoners.
Committee to consider the question of the need of some special training for Road Engineers.
Nomenclature Committee, constituted by The Institute of Metals.
(In suspense during the War.)
Executive Committee of the Engineering Institutions' Volunteer Engineer Corps.
Grand Committee of the City of Westminster Volunteers.
Also at the opening of the new Buildings of the Massachusetts Institute of Technology.

Calcutta and District Section.

The Calcutta and District Section of the Institution held Meetings during the Session 1915–16 at which the following local Papers were read :—

Fire Prevention and Extinction; by B. A. Westbrook. (November.)
Automatic and Semi-Automatic Machine-Tools; by C. Warren Boulton. (December.)
Manufacture of Oxygen and its use for Welding and Metal-cutting; by J. M. Christie, *Member*. (January.)

Short Abstracts of these Papers have been printed in the Proceedings (Proceedings 1916, page 865).

At the February Meeting the Chairman read the 1915 "Thomas Hawksley" Lecture on "The World's Supplies of Fuel and Motive Power," by Dr. Dugald Clerk, F.R.S.

Visits were made to the following Works:—

Eastern Bengal Railway Shops at Kanchrapara.
Shipyard of the Indian General Navigation and Railway Company, at Raja Bagan.

The Annual Dinner of the Section was held on 15th January. Eighty-eight members of the Institution are on the Roll of the Section, of whom Mr. G. B. Williams has acted as Chairman, with Mr. B. W. Bagshawe as Honorary Secretary and Mr. J. H. H. Rolfe as Honorary Treasurer of the Section. A grant of £25 towards the expenses in 1916 has been made from the Institution Funds. Assistance was rendered to the Government of India in the organization of the manufacture of munitions in India, and the Committee of the Calcutta Section visited different portions of the district and submitted a list of lathes which assisted very largely in stocking the shell factory in Bengal. At the request of the Munitions Branch of the Government of India, the Section also compiled a register for all India of suitable men for employment on the manufacture of munitions.

Graduates.

Owing to the abnormal condition of the times, the Graduates' Meetings were suspended during the year, but the Engineering Societies of the Universities of Birmingham, Bristol, Edinburgh, Glasgow, and Manchester, and of the University College of South Wales and Monmouthshire, and Armstrong College, Newcastle-on-Tyne, kindly repeated their invitation to the Graduates residing in those neighbourhoods to attend their Meetings during the Session.

Benevolent Fund.

The Benevolent Fund have made grants to several members and dependents of members of the Institution. A donation from Mr. H. E. Jones of Fifty Guineas was much appreciated, being his Fee for the delivery of the "Thomas Hawksley" Lecture. Ninety Life Members have made donations of Ten Guineas and upward, and 149 Ordinary Members are Annual Subscribers of not less than Half a Guinea. It is to be much regretted that a large number of members of the Institution have not yet subscribed. It has been impracticable to vary the Institution Memorandum of Association with the object of enabling a grant to be made to the Fund as had been suggested.

Dr. ACCOUNT OF REVENUE AND EXPENDITURE

Expenditure.

	£ s. d.	£ s. d.
To Expenses of Maintenance and Management—		
Salaries and Wages	4,319 14 5	
Postages, Telegrams, and Telephone	716 18 11	
Heating, Lighting, and Power	45 19 5	
Fittings and Repairs	22 8 7	
Housekeeping	19 3 3	
Incidental Expenses	51 5 5	
		5,175 10 0
,, Printing, Stationery, and Binding—		
Printing and Engraving Journal	1,364 3 4	
Stationery and General Printing	723 12 0	
Binding	82 7 6	
		2,170 2 10
,, Rent, Rates, Taxes, &c.—		
Ground Rent (Birdcage Walk)	1,107 17 2	
Do. No. 5 Princes Street	135 0 0	
Rent of temporary Offices	650 0 0	
Warehouse Rent	13 10 0	
Insurance	13 6 2	
		1,919 13 4
,, Meeting Expenses—		
Printing	113 16 8	
Travelling and Incidental Expenses	71 6 2	
Reporting	33 12 0	
Local Meetings	44 18 10	
		263 13 8
,, Calcutta and District Section		25 0 0
,, Council Prizes		18 14 6
,, Books purchased		45 2 1
,, Law Charges		16 7 6
,, Expenses of Examinations		111 14 0
,, Expenses in connexion with Research Committees		594 12 4
,, Donation to Engineering Standards Committee		500 0 0
,, Depreciation on Furniture and Fittings		177 14 8
,, Debenture Interest		1,361 0 0
,, Interest on Loan		249 14 0
Total Expenditure		12,628 18 11
Balance, being excess of Revenue over Expenditure (exclusive of value of Subscriptions in arrear), carried to Balance Sheet		9,815 2 9
		£22,444 1 8

FOR THE YEAR ENDED 31ST DECEMBER 1916. *Cr.*

Revenue.

	£	s.	d.
By Subscriptions for 1916	13,988	0	0
„ Subscriptions in arrear, paid in 1916	1,158	0	0
„ Rents of Institution Building	5,900	0	0
„ Interest, &c. (exclusive of Trust Funds)—			
From Investments and Deposits at Bank	478	15	5
„ Sale of Proceedings, Journals, and Bound Volumes—			
Extra Copies sold	272	0	3
„ Examination Fees	27	6	0
„ Grants for Research—			
From the Government (part for 1917) . . 600 0 0			
From the Bryan Donkin Fund 20 0 0			
	620	0	0
	£22,444	1	8

Dr. BALANCE SHEET

	£ s. d.	£ s. d.
To 4% Debentures (1912 Issue):—		
327 *Debentures of* £100 *each*	32,700 0 0	
23 *Debentures of* £50 *each*	1,150 0 0	
7 *Debentures of* £25 *each*	175 0 0	
		34,025 0 0
„ Sundry Creditors—		
Loan from Messrs. Richard and Robert H. Williamson	6,000 0 0	
Accounts owing at 31*st Dec.* 1916 . . .	1,729 0 11	
Unclaimed Debenture Interest (coupons not presented)	590 19 11	
		8,320 0 10
„ Subscriptions paid in advance		151 10 0
„ Pensions Contingencies Fund (*Investment and Interest*)		316 18 4
„ Sinking Fund set aside for Redemption of Debentures and Institution's Leasehold Property		19,083 8 11
„ Balance, being Capital of the Institution, exclusive of the Sinking Fund:—		
Balance at 31*st Dec.* 1915	76,373 9 9	
Deduct— £ s. d.		
Amount set aside for Redemption of Debentures and Institution's Leasehold Property . . . 10,762 0 0		
Amount set aside for Pensions Contingencies Fund 100 0 0		
	10,862 0 0	
	65,511 9 9	
Add—		
Excess of Revenue over Expenditure for the year ended 31*st Dec.* 1916 .	9,815 2 9	
Amount received from Entrance Fees during 1916	286 0 0	
Amount received from Life Compositions during 1916	79 0 0	
		75,691 12 6
		137,588 10 7
„ Trust Funds (see pages 20-21), *per contra*		4,119 9 5
		£141,708 0 0

Signed by the following Members of the Finance Committee:—

MARK ROBINSON (*Chairman*).
W. C. UNWIN.
W. H. MAW.
JOHN DEWRANCE.
H. S. HELE-SHAW.
H. A. IVATT.
WILSON WORSDELL.

AT 31st DECEMBER 1916. *Cr.*

	£ s. d.	£ s. d.
By Cash (exclusive of Trust Funds)—		
In Union of London and Smiths Bank—		
Deposited at Interest	2,700 0 0	
On Current Account	774 1 4	
„ Sinking Fund Account	434 2 5	
	3,908 3 9	
In the Secretary's hands	80 16 4	
		3,989 0 1
„ Amount in Union of London and Smiths Bank to meet unclaimed Debenture Interest (*coupons not presented*)		590 19 11
„ Investments (*of which £19,083 8s. 11d. has been set aside for Redemption of Debentures and the Institution's Leasehold Property, and £311 6s. 3d. for the Pensions Contingencies Fund*) *Cost*		29,275 9 8
£ s. d.		
6,500 0 0 *Registered 5% Exchequer Bonds* 1920.		
4,500 0 0 *Do.* 1921.		
5,408 0 0 *L. and N. W. Ry. 3% Debenture Stock.*		
5,080 12 0 *Midland Ry. 2½% Debenture Stock.*		
4,806 2 2 *War Stock 4½% 1925-45.*		
3,084 10 8 *Metropolitan Water (B) 3% Stock.*		
1,586 0 0 *London County 3½% Consolidated Stock.*		
1,370 0 0 *L. & S. W. Ry. 3% Consolidated Deb. Stock.*		
The above nominal values are subject to depreciation, but, in view of the continuance of the War, no estimate of present values can usefully be made.		
„ Subscriptions in arrear, *not valued.*		
„ Furniture and Fittings (*less depreciation*)		1,599 12 4
„ Books in Library, Drawings, Engravings, Models, Specimens, and Sculpture (*estimate of* 1893)		1,340 0 0
„ Proceedings—stock of back numbers, *not valued.*		
	£ s. d.	
„ Institution House *Cost*	60,270 2 10	
„ Princes Street Extension „	40,523 5 9	
		100,793 8 7
		137,588 10 7
„ Trust Funds Investments and Cash in Bank (see pages 20–21), *per contra*—	£ s. d.	
Willans Premium Fund	170 0 4	
Water Arbitration Prize Fund	537 8 10	
Bryan Donkin Fund	369 16 8	
Starley Premium Fund	480 10 9	
Thomas Hawksley Fund	2,561 12 10	
		4,119 9 5
		£141,708 0 0

I have examined the above Balance Sheet and report that I have obtained all the information and explanations I have required. In my opinion such Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Institution's affairs according to the best of the information and explanations given to me and as shown by the Books of the Institution.

RAYMOND CRANE, F.C.A.,
Auditor,
46/47 London Wall, London, E.C.

15th January 1917.

WILLANS PREMIUM FUND.

(*Subscribed*, 1897, *by friends of the late P. W. Willans, to commemorate the services he rendered to Engineering and Electrical Science.*)
(Under a Joint Trust with The Institution of Electrical Engineers.)

	£	s.	d.	£	s.	d.
Investment £159 8s. 5d. of India 3% Stock . . . . . *cost*				165	5	0
By Cash Balance from 1915	14	6	0			
,, Interest received in 1916	4	15	4			
	19	1	4			
To Award by The Institution of Electrical Engineers .	14	6	0			
				4	15	4
Total, as per Balance Sheet				£170	0	4

(For Declaration of Trust, *see* Proceedings 1913, page 124.)

WATER ARBITRATION PRIZE FUND.

(*Presented by Sir Edward Fry and the Metropolitan Water Board, to commemorate the holding, in the Institution building, of the Court of Arbitration*, 1903–5, *relating to the purchase of the Undertakings of the eight London Water Companies.*)

	£	s.	d.	£	s.	d.
Investment £523 10s. 2d. of Metropolitan Water (B) 3% Stock *cost*				500	0	0
By Cash Balance from 1915	22	12	9			
,, Interest received in 1916	12	11	1			
,, Income Tax refunded in 1916	2	5	0			
				37	8	10
Total, as per Balance Sheet				£537	8	10

(For Regulations, *see* page 22.)

BRYAN DONKIN FUND.

(*Bequest*, 1909, *under the Will of the late Bryan Donkin, Vice-President* 1901–2.)

	£	s.	d.	£	s.	d.
Investment £349 15s. 8d. of London County 3½% Consolidated Stock . . . . . *cost*				360	6	6
By Cash Balance from 1915	33	5	7			
,, Interest received in 1916	9	12	4			
,, Income Tax refunded in 1916	1	12	3			
	44	10	2			
To Grant to The Institution of Mechanical Engineers for Research . .	20	0	0			
,, ,, ,, Mr. A. H. Barker	15	0	0	35	0	0
				9	10	2
Total, as per Balance Sheet				£369	16	8

(For Declaration of Trust, *see* page 22.)

STARLEY PREMIUM FUND.

(*Presented*, 1906, *by the J. K. Starley Memorial Committee.*)

	£	s.	d.	£	s.	d.
Investment £435 8*s.* 5*d.* of London County 3½% Consolidated Stock . . . . . *cost*				440	7	6
By Cash Balance from 1915	26	3	7			
,, Interest received in 1916	11	19	6			
,, Income Tax refunded in 1916	2	0	2			
				40	3	3
Total, as per Balance Sheet				£480	10	9

(For Declaration of Trust, *see* Proceedings 1916, page 25.)

THOMAS HAWKSLEY FUND.

(*Established by Mr. Charles Hawksley, Member of Council, to commemorate the Centenary on 12th July* 1907 *of the birth of his Father, the late Mr. Thomas Hawksley, F.R.S., President* 1876–1877 (*died* 23 *Sept.* 1893).)

Capital—	£	s.	d.	£	s.	d.
£1,300 *Southend Waterworks Co.* 4% *Perpetual Debenture Stock . . . . nominal value*	1,300	0	0			
£700 *Lowestoft Water and Gas Co.* 4% *Debenture Stock . . . . . nominal value*	700	0	0			
Accumulated Interest (invested)—						
£420 *G. W. Ry.* 4½% *Debenture Stock . cost*	499	14	6			
				2,499	14	6
By Cash Balance from 1915	48	16	9			
,, Interest received in 1916	81	1	7			
,, Income Tax refunded in 1916	13	2	8			
	143	1	0			
	£	s.	d.			
To Lecturer, 1916	52	10	0			
,, Expenses of London and Provincial Meetings	18	12	8			
,, Premiums, Prof. A. H. Gibson and Mr. W. J. Walker .	10	0	0			
	81	2	8			
				61	18	4
Total, as per Balance Sheet				£2,561	12	10

(For Declaration of Trust, *see* Proceedings 1915, page 25.)

Water Arbitration Prize.

Regulations for the Sixth Award, to be made in February 1919.

1. The Award will be made for the best original Paper dealing with any of the following subjects:—Methods of Purification of Water for Domestic or Trade Purposes, or Description of Hydraulic Machinery, or New Investigations in Hydraulics, or New Developments in Distribution of Water for Town's Supply or Irrigation, or Advances in the Utilization of Water Power, accepted by the Council for publication with or without discussion in the Institution Proceedings of 1917 and 1918, provided that the Paper be of sufficient merit in the judgment of the Council.

2. Papers should be sent in as soon as possible, but not later than 1st April 1918.

3. Papers should be illustrated by scale drawings, but may be accompanied by photographs, lantern-slides, and specimens.

4. Any Paper not accepted for printing in the Proceedings will be returned to the Author.

5. The Prize will have the value of about £30, and will be accompanied by a Certificate bearing the Seal of the Institution.

Bryan Donkin Fund.

Declaration of Trust.

To all to whom these Presents shall come The Institution of Mechanical Engineers (hereinafter called "the Institution") send greeting.

Whereas the late Bryan Donkin became a Member of the Association called The Institution of Mechanical Engineers in the year 1873 and was a Member of the Council of the Institution from the year 1895 to the year 1901 when he became a Vice-President of the Institution And whereas the said Bryan Donkin died on the 4th day of March 1902 having by his Will dated the 22nd day of March 1894 and proved in the Principal Probate Registry on the 25th day of March 1902 bequeathed to the

Institution a Legacy of £300 and thereby expressed the desire (but not so as to create any trust) that the Managing Body for the time being of the Institution should if possible devote the Legacy towards original research AND WHEREAS the said Legacy and a sum being the interest thereon together amounting to £360 6*s.* 6*d.* was paid by the Executors of the said Bryan Donkin to the Institution in the month of June 1908 and was invested in the name of the Institution in an amount of £349 15*s.* 8*d.* London County 3½ per cent. Consolidated Stock AND WHEREAS the Council of the Institution (hereinafter called "the Council") are the managing body of the Institution and "Bryan Donkin" Awards out of income of the said investment were made by the Council in the years 1910, 1913 and 1916 and a fourth Award is to be made in February 1919 on terms already published and in accordance with a resolution of the Council the Institution have determined to hold the said fund after the 28th day of February 1919 upon trust for the purposes hereinafter mentioned NOW THESE PRESENTS WITNESS that the Institution do hereby declare that from and after the said 28th day of February 1919 they will hold the said sum of Stock upon the trusts and with and subject to the powers and provisions hereinafter appearing namely:—

1. THAT the said sum of Stock (with any additions from time to time made to the capital thereof) shall remain or be invested in the name of the Institution in or upon any investments for the time being authorised under the Articles of Association of the Institution for the investment of moneys of the Institution or in or upon any investments permitted by law for the investment of trust funds and that the Institution may at any time and from time to time vary such investments for others of a like nature.

2. THAT the said sum of Stock (with any such additions) and the investments for the time being representing the same (hereinafter referred to as "the Trust Fund") shall be called the "Bryan Donkin Fund" and shall be kept as a separate account in the books of the Institution.

3. THAT out of the income of the Trust Fund there shall first be paid all costs and expenses of or incidental to the management

of the Trust Fund and the income thereof or the execution of the trusts or powers of these presents.

4. That subject to Clause 3 hereof the said income shall be applied by the Council at the end of triennial periods subsequent to and dating from the month of February 1919 in grants to be called the "Bryan Donkin Awards" in aid of original research in the science or practice of Mechanical Engineering. And Notice of the next offer of Awards shall be published by the Council in the Journal or Proceedings of the Institution previously to the 31st July in the year immediately preceding the year of the next Awards.

5. Applications for grants must be sent in writing to the Secretary of the Institution and received at the Institution on or before the 31st October in the year immediately preceding the year of the next Awards. Each application must be accompanied by the names and addresses of two references as to the suitability and the ability of the applicant to undertake the proposed research, and must contain a concise statement as to the nature of the research and the sum applied for, with details of the proposed expenditure.

6. All applications received in accordance with Clause 5 shall be considered by the Council on a report thereon from the Research Committee of the Council. And the Council in their sole discretion shall decide as to the allocation of the sum available and as to what grants (if any) shall be made to any of the applicants. And if it is considered that none of the applicants is deserving of a grant or if the grants made amount to less than the sum available the said income or the surplus thereof shall be devoted to aiding any of the researches of the Institution and shall be distributed in such proportions as the Council shall decide.

7. The full amount of each grant made by the Council shall be handed over to the applicant at his request but upon the express conditions hereinafter provided. If no request for payment is received within six months the grant shall lapse.

8. Any portion of a grant not expended by 31st December in each year shall forthwith be returned to the Institution unless permission in writing has been previously received from the Council for its retention, in order to continue the research.

9. The recipient of a grant shall send to the Institution on each 1st January during the continuation of his research a Report of his progress with particulars of the expenditure incurred accompanied as far as possible by vouchers. He shall also at the request of the Council send to the Institution a statement of his experiments and the results obtained; and the Council shall be at liberty to publish the same or any part thereof in the Journal or Proceedings of the Institution.

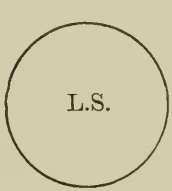
10. Any recipient of a grant who fails to fulfil the conditions in Clause 9 or whose Report is not considered satisfactory by the Council may be required to forthwith return to the Institution the whole of the amount granted to him.

11. Any sum so returned to the Institution may at the discretion of the Council be applied as if it were income available for the next triennial grants or be devoted to aiding any of the said researches of the Institution.

12. The Institution at any time and from time to time upon the recommendation of the Council may by deed under their Common Seal wholly or partially revoke or alter all or any of the trusts powers or provisions hereinbefore contained And may in lieu thereof declare such new or altered Trusts with and subject to such new or altered powers and provisions in relation to the Trust Fund and the income thereof as the Institution upon such recommendation as aforesaid shall think proper.

In witness whereof the Institution have caused their Common Seal to be hereunto affixed this tenth day of April One thousand nine hundred and sixteen.

The Common Seal of The Institution of Mechanical Engineers was hereunto affixed in the presence of



W. Cawthorne Unwin, *President.*
Mark Robinson, *Vice-President.*
Edgar Worthington, *Secretary.*

LIST OF ADDITIONS TO THE LIBRARY.

BOOKS, *see p.* 26.
OFFICIAL PUBLICATIONS, *see p.* 28.
PAMPHLETS, *see p.* 30.
DIRECTORIES, ANNUALS, &c., *see p.* 32.
CALENDARS and COLLEGE REPORTS, *see p.* 32.
PUBLICATIONS OF SOCIETIES, &c., *see p.* 33.
PERIODICALS, *see p.* 37.

BOOKS (*in order received*).

The Flying Machine—Two Papers: The Aerofoil and the Screw Propeller, by F. W. Lanchester; from the author.

Aero Engines (3rd ed.), by G. A. Burls.

Test Methods for Steam Power Plants, by E. H. Tenney.

Internal Combustion Engines, by R. L. Streeter.

Irrigation Practice and Engineering, Vols. I and II, by B. A. Etcheverry.

Overhead Transmission Lines and Distributing Circuits, by F. Kapper (translated by P. R. Friedlaender).

Three volumes of MS. Notes by the late F. W. Crohn of his Technical Education at the Karlsruhe Polytechnic, 1850–53 (German); from Mr. G. Schulze.

Aircraft in Warfare: The Dawn of the Fourth Arm, by F. W. Lanchester (a second copy has been received from the author).

Modern Workshop Practice (2nd ed.), by E. Pull; from the publishers.

The Arguments of the Judges in Two Causes relating to the Letters Patent granted to James Watt, Engineer, for his method of Lessening the Consumption of Steam and Fuel in Fire Engines (1799); from Mr. Walter Powell.

Shrapnel and other War Material (a reprint of articles in the "American Machinist"), by Captain Duchêne.

The Mechanics of the Aeroplane (translated from the French by J. H. Ledeboer and T. O'B. Hubbard).

Napier Tercentenary Memorial Volume, edited by C. G. Knott; from the Royal Society of Edinburgh.

The Growth of Cities in the Nineteenth Century—A Study in Statistics, by A. F. Weber, Ph.D.; from Mr. R. Price-Williams.

The A.B.C. Guide to Patents for Inventions (2nd ed.), by R. E. Phillips and A. M. Flack; from Mr. R. E. Phillips.

Machinery's Mechanical Library:—Vol. I, Turning and Boring, by F. D. Jones; Vol. II, Planing and Milling, by F. D. Jones; Vol. III, Drilling Practice and Jig Design, by E. Oberg and F. D. Jones; Vol. IV, Advanced Grinding Practice, by D. T. Hamilton and F. D. Jones; Vol. V, Modern Toolmaking Methods, by F. D. Jones; Vol. VI, Diemaking and Die Design, by F. D. Jones; Vol. VII, Tools, Chucks and Fixtures, by A. A. Dowd; Vol. VIII, Heat-Treatment of Steel, by E. Oberg; Vol. IX, Spur and Bevel Gearing, by E. Oberg; Vol. X, Spiral and Worm Gearing; from the publishers.

Steam Power, by Professor William E. Dalby, F.R.S.; from the Kershaw Donation.

The Callendar Steam Tables, by Professor H. L. Callendar, F.R.S.

Land and Marine Diesel Engines, by G. Supino (translated by A. G. Bremner and J. Richardson).

Elementary Sanitary Engineering for Indian Sanitary Inspectors, by G. B. Williams; from the author.

Transactions of the International Engineering Congress, 1915, San Francisco: Mechanical Engineering Volume and Index Volume.

The Civil Engineers' Cost Book (2nd ed.), by Lt.-Col. T. E. Coleman; from the publishers.

Experiments on the Elastic Deformations of Autogenous (Oxy-Acetylene) Welds and of Welded Drums, by S. H. Barraclough and H. A. Rorke; from Professor S. H. Barraclough.

Boiler Explosions as Affected by Unsymmetrical Riveted Joints, by S. H. Barraclough, A. J. Gibson, H. W. May, and E. P. Norman; from Professor S. H. Barraclough.

Machine Design (4th ed.), by A. W. Smith and Professor G. H. Marx.

Alternating Currents—Their Elements explained, and their Calculation Effected without the Use of Hyperbolic Functions, by H. R. Kempe; from the author.

Elementary Applied Mechanics (3rd ed.), by T. Alexander and A. W. Thomson.

The Structure and Properties of the more common Materials of Construction, by G. B. Upton.

La Science Française, Vols. 1 and 2; from the Director, Office National des Universités et Écoles françaises.

The Metallography and Heat Treatment of Iron and Steel (2nd ed.), by Albert Sauveur.

De Columna Traiani, by Raphaelis Fabretti (1683).

The Heat Treatment of Tool Steel (2nd ed.), by Harry Brearley; from the publishers.

Mechanical Draft: A Practical Treatise (3rd ed.), edited by W. B. Snow; from Mr. George A. Mower.

Heating and Ventilation; from Mr. George A. Mower.

The Air Supply to Boiler Rooms of Modern Ships of War, by R. W. Allen; from the author.

Economy in Bridge Design and Construction, by H. W. Joyce; from the Civil Engineering College, Sibpur.

Practical Mathematics for Technical Students (Parts I and II), by T. S. Usherwood and C. J. A. Trimble; from Mr. T. S. Usherwood.

The Design of Aeroplanes, by A. W. Judge.

The Metallurgy of Steel (2 vols., 5th ed.), by. F. W. Harbord and J. W. Hall.

The Mechanical Handling and Storing of Material, by G. F. Zimmer.

Aeronautical Engines, by F. J. Kean; from the publishers.

Typographical Printing-Surfaces: the Technology and Mechanism of their Production, by L. A. Legros and J. C. Grant; from Mr. L. A. Legros.

The Principles of Apprentice Training with special reference to the Engineering Industry, by A. P. M. Fleming and J. G. Pearce.

Oil-Field Development and Petroleum Mining, by A. Beeby Thompson; from the author.

Official Guide for Shippers and Travellers in the Orient, by the Osaka Shosen Kaisha; from the publishers.

The Physico-Chemical Properties of Steel, by C. A. Edwards.

Wire and Steel Gauge Tables, by Thomas Stobbs; from the publishers.

Worm Gearing (2nd ed.), by H. Kerr Thomas; from the author.

The Panama Canal (2 vols.), by Major-General G. W. Goethals, U.S. Army.

OFFICIAL PUBLICATIONS.

British Isles.

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*Brotherhood of Locomotive Firemen's Magazine.
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Electrical Engineers, Institution of; Journal.
Engineers and Shipbuilders in Scotland; Transactions.
Iron and Steel Institute; Journal.
Liverpool Engineering Society; Transactions.

Metals, Institute of; Journal.
North-East Coast Institution of Engineers and Shipbuilders; Transactions.
South Wales Institute of Engineers; Proceedings.
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Additions to Museum.

Framed Oil Painting of the Steamboat of Jonathan Hulls, 1736.
Framed Engraving of George Stephenson, by Charles Turner, 1838.
Piece of Framework of Zeppelin brought down at Salonika, from Lieut. W. E. Wort (*M.*).

The Institution of Mechanical Engineers.

PROCEEDINGS.

January 1917

An Ordinary General Meeting was held at The Institution of Civil Engineers, London, on Friday, 19th January 1917, at Six o'clock p.m.; Dr. W. Cawthorne Unwin, F.R.S., *President*, in the Chair.

The Minutes of the previous Meeting were read and confirmed.

The President announced that, in accordance with Article 23, the President, two Vice-Presidents, and seven Members of Council would retire at the ensuing Annual General Meeting; and the complete list of those retiring was as follows:—

President.

W. Cawthorne Unwin, LL.D., F.R.S., . .	London.

Vice-Presidents.

Edward Hopkinson, D.Sc., ...	Manchester.
Mark Robinson, ...	London.

Members of Council.

Dugald Clerk, D.Sc., F.R.S., . . .	London.
Sir A. Trevor Dawson, ...	London.

Sir ROBERT A. HADFIELD, D.Sc., D.Met., F.R.S.,	Sheffield.
GEORGE HUGHES,	Horwich.
ROBERT MATTHEWS,	Manchester.
D. B. MORISON,	Hartlepool.
Sir GERARD A. MUNTZ, Bart.,	Birmingham.

The following were hereby nominated by the Council for election, with their consent:—

For PRESIDENT.

MICHAEL LONGRIDGE,	Altrincham.

For VICE-PRESIDENTS.

DUGALD CLERK, D.Sc., F.R.S.,	London.
Sir ROBERT A. HADFIELD, D.Sc., D.Met., F.R.S.,	Sheffield.
MARK ROBINSON,	London.

For MEMBERS OF COUNCIL.

Sir A. TREVOR DAWSON,	London.
GEORGE HUGHES,	Horwich.
ROBERT MATTHEWS,	Manchester.
D. B. MORISON,	Hartlepool.
Sir GERARD A. MUNTZ, Bart.,	Birmingham.

The following Nominations were also made by the Council for election at the Annual General Meeting, with their consent:—

For MEMBERS OF COUNCIL.

Election as Member.		
1907.	WILLIAM REAVELL,	Ipswich.
1903.	DAVID E. ROBERTS,	Cardiff.
1883.	RICHARD WILLIAMSON,	Workington.

The PRESIDENT reminded the Meeting that, according to By-law 26, any Member or Associate Member was then entitled to add to the list of candidates.

No other names being added, the PRESIDENT announced that the foregoing names would accordingly constitute the nomination list for the Election of Officers at the Annual General Meeting.

The PRESIDENT announced that the following seven Transferences had been made by the Council:—

Associate Members to Members.

BENFIELD, JOHN, Lieut., A.O.D., . .	Leicester.
CRUICKSHANK, GEORGE SEYMOUR. . .	Peking.
GILLETT, THOMAS, ...	Leatherhead.
MOHN, TRYGVE, ...	Ashton-under-Lyne.
POWELL, WILLIAM HENRY, . . .	Llanelly.
RICHARDS, GEORGE TILGHMAN, . . .	Dalmuir.
ROBERTSON, WALTER HENRY ANTONIO, .	Bedford.

The following Paper was read and discussed:—

"The Manufacture of Gauges at the L.C.C. Paddington Technical Institute"; by A. G. COOKE, M.A., W. J. GOW, A.R.C.S., and W. G. TUNNICLIFFE, *Associate Member*, of London.

The Meeting terminated shortly after Half-past Seven o'clock. The attendance was 63 Members and 49 Visitors.

The Paper on "The Manufacture of Gauges" was presented by Mr. Cooke and further discussed at:—

LEEDS, in the Philosophical Hall, Park Row, on Wednesday, 24th January 1917. Mr. J. HARTLEY WICKSTEED, *Past-President*, presided.

BIRMINGHAM, in the Medical Lecture Theatre of the University, Edmund Street, on Thursday, 25th January 1917. Mr. WALTER DEAKIN, *Member*, presided.

THE MANUFACTURE OF GAUGES AT THE L.C.C. PADDINGTON TECHNICAL INSTITUTE.

By A. G. COOKE, M.A., W. J. GOW, A.R.C.S., and W. G. TUNNICLIFFE, *Associate Member.*

Although the Authors make no affectation of secrecy with regard to the ultimate purpose of the work described in this Paper, it will be wise to avoid, not only matters of organization and finance, but even reference to any other agency concerned, whether authoritative or auxiliary. Nor do they pose as scientific experts, after eighteen months of auxiliary labour, on a matter that requires a lifetime of study. In the standardization of an engineering industry, vastly exceeding in magnitude any previous human experience, they have been honoured with an important share in practical constructive work, not on account of any recognized merit or skill, but the accident of being the largest Technical Institute maintained by the largest Education Authority.

The Authors deal with workshop details and methods of securing a high degree of mechanical accuracy, under conditions of great urgency and without special equipment. As this work would differ in no particular from that required in the standardization of

any engineering industry of great magnitude, they may hope that an account of their experience may not be without value when another great national effort is required to restock the world with the munitions of civilization.

This is an Institute devoted to the technical education of the artisan and foreman. The workshop staff is exclusively composed of teachers engaged in the early training of this class, and the pupil assistants are boys representative of the intermediate training. A severe practical test of their efficiency has been made, and their claim is that they have justified the confidence of engineers, even though they may have contributed little that is novel to the science of workshop accuracy.

At the close of the session for evening classes in 1915, the personnel of the Mechanical Engineering Department of the L.C.C. Paddington Technical Institute was reduced to the Head, who, with the Principal, was continuing the engineering teaching of a Junior Technical School of 100 boys (14–16). Of the five permanent teachers of engineering, four had joined the Army and the fifth was "on loan" to a munition factory. From the elementary schools of the Council were transferred two metal-work instructors who had experience in gauge-making, Mr. W. G. Tunnicliffe and Mr. H. C. Christie. Under their immediate direction were placed five other metal-work instructors and sixteen selected woodwork instructors.

Boys of the Junior Technical School in their second year were also employed; the ultimate arrangement adopted being for classes of about seventeen to work every third week for forty-four hours. Experience confirms that this is excellent in results, and worthy of consideration as a permanent system of engineering training.

Machinery that had been in ceaseless use by day and evening students for ten to a dozen years could hardly be expected to be ready for work of the highest accuracy. But no other was available. Practically for all but the "roughing-out" machines complete reconstruction of those essential lathe parts on which precision depends was the first operation.

Nature of Manufacture.—An explanation is necessary as to the nature of the work undertaken, and its place in relation to the whole industry. The precision desired for primary standards for laboratory testing is limited by practical possibilities only, and the manufacture is not a "commercial proposition." Inspection gauges, or secondary standards, are to be manufactured within a determined though small range of variation or "tolerance." This tolerance should be regarded as a definition, not an error limit. Within the tolerance all values are "correct." In consequence, the manufacture is entirely a question of competitive cost and urgency of delivery.

That standardization is the key to efficient manufacture and a large output is universally recognized. It is not so well understood that simply on account of the magnitude of the output, apart from the question of excellency of workmanship required, the necessity for standards of a higher order of accuracy arises. It is easy in a small workshop to insure by trial that all fitting is satisfactory; in fact, the initial magnitudes are generally determined by the same tools and standards. In a larger workshop the work is linked by more accurate gauges; and it generally matters little if the absolute measurements vary appreciably from that specified in the design. But when an industry grows to a national or international extent, the standards must aim at specified dimensions of absolute magnitude, defining exactly the limits of permissible variation on either side of the standard, so as to include all acceptable and reject all unacceptable variations from the correct value. In the language of the calculus, in order to limit variations to a defined "small quantity," inspection gauges are required to be true to the "second order of small quantities." It is just in so far as we can rely on the accuracy of inspection gauges to define the limits of variation permissible, that the designer can allow those limits to be extended with safety. Hence the apparent paradox that high accuracy in the inspection gauges allows more laxity, and so increases facility and speed of manufacture.

Limit-Gauges.—There is no need to describe in detail the

principles of limit-gauges for measurements of length and diameters. Both workshop and inspection gauges were manufactured, the essential difference being that the former must be adjusted to be within the specified limits, while any tolerance permissible in the latter must be outside, so as to pass all acceptable work. Tolerance in a workshop gauge tends only to increase the margin of safety, enforcing greater care in manufacture than necessity demands. Uncertainty and variation in inspection gauges is not only unjust to one manufacturer, but affects all by necessitating closer limits.

The necessary order of accuracy for this type of gauge, three ten-thousanths is generally specified, can be obtained with universal grinders. As the demand was urgent, and grinding machinery not available, they had to resort to handwork. Lapping with high-speed motors up to three or four thousand revolutions, and other devices for acceleration, were developed.

Position-Gauges.—The greater part of the work entrusted to this Institute consisted of a class of gauges determining position or some geometrical feature, involving greater difficulties in theory and practice than a single directly measured dimension. Concentricity, perpendicularity, or parallelism, and distance between centres of turning and drilling, are examples. The Authors are unfortunately unable to give details, but it will be seen that to determine some such geometrical condition, or primary measurements, in the work to be tested, limits of accuracy are allowed by a definite variation in the size of diameters of cylinders, pins, or drillings, so as to allow slackness. The gauge limits the combined errors in jig or automatic tool work.

It will be seen that any variation of the inspection gauge, as regards the primary condition, simply displaces the limits which it is intended to give. rejecting satisfactory and accepting unsatisfactory work through a range of double its own error. The practice is to allow a gauge tolerance of only 0·0001 in the primary measurement, and a combined tolerance of 0·0003.

Screw-Gauges.—The methods by which they attained the high accuracy required for screw-gauges are selected for special description. They try to limit any discussion of scientific principles to such as directly affect their work in the construction of the gauges. In screw-gauges the gauging surface is an elaborate skew surface, all parts of which are required to be within the limits, in general fixed at three ten-thousandths measured radially from the axis [0·0006 in diameter]. This means only about 1½ ten-thousandths normally to the slope of the threads. It must not be forgotten that this is the second order of small quantities necessary to define a larger variation permissible in the work. As a variation of which this is five per cent. would allow a sheet of the *Times* newspaper to be used as packing (three thousandths), it cannot be regarded as too fine.

It should be evident that the accuracy in pitch of the lathe used should be such that no divergency from the correct value greater than about 1½ ten-thousandths should exist throughout the range used; for this would cause a skew surface otherwise cut in correct shape to the mean value to pass outside the limits allowed. If any further error in pitch exists, up to an extreme value of three ten-thousandths, it is only possible to keep within limits by thinning of threads, reducing the effective diameter, and the practical difficulties of the reduced margin increase rapidly if the above error is exceeded. The Authors found that the lathes by a number of manufacturers showed an error in pitch of about two in a thousand, always short of specification. This uniform or progressive error is fairly easily corrected by change of gear-wheels, as will be shown by an example, but indicates the lamentable need for standardization in leading screws. Superimposed on this error were periodic errors, initial and produced by wear, requiring measurement, analysis and correction. It is desirable to repeat that they are aiming not at perfection, but a precision defined by tolerance. In a choice between methods of correction, that which will permanently rectify a discovered cause of variation so as to bring it within the defined limits, is infinitely preferable to any method of compensation which, though more capable of

refinement, would allow redevelopment of error in prolonged heavy use, thus delaying output.

The periodic error is amenable to reduction by a final lapping process, but for this not only the amplitude but the length of the periods must be considered, and probably also the method of lapping. Short periods and lapping with high speeds through several turns of the lap seemed necessary to success.

Workshop Measurements.—It was found absolutely necessary to design and make for themselves instruments by which measurements of pitch and the diameters of the screw-gauges could be made on the premises, and optical apparatus by which screw-cutting-tools could be shaped and the sections of the screw-threads examined. Details of these may perhaps be of interest, not as examples of high accuracy of physical measurement, but of practical methods suitable to the workshop.

The apparatus for pitch measurement is shown in Fig. 1. By its use on test-pieces they were enabled to analyse step by step the errors of pitch in lathes. One example will be given in some detail. The pin, which is moved from groove to groove, followed by micrometer, is turned to a cone of angle slightly less than 55°, and the point removed to ensure that the pin touches, not the bottom, but the sides of the groove. Supreme care, by good fitting, is necessary to insure that the points of contact follow a line parallel to the axis of the screw.

The differences between the micrometer readings for each groove, and those calculated for a correct screw of the presumed pitch, are plotted as ordinates in Figs. 2 and 3 (pages 52–3). These are selected from a large store of records as the first and final pitch diagrams of test-pieces from the same lathe before and after correction. The test of Fig. 3 was taken on opposite sides of a test-piece of fourteen threads to the inch, the leading screw-pitch being presumed four threads to the inch. Analysed, it shows a fairly uniform progressive error of 0·0022 per inch, with superimposed periodic errors corresponding to the revolution of the leading screw, of amplitude 0·0004 inch. The lathe was in good condition, fairly typical of ordinary workshop accuracy.

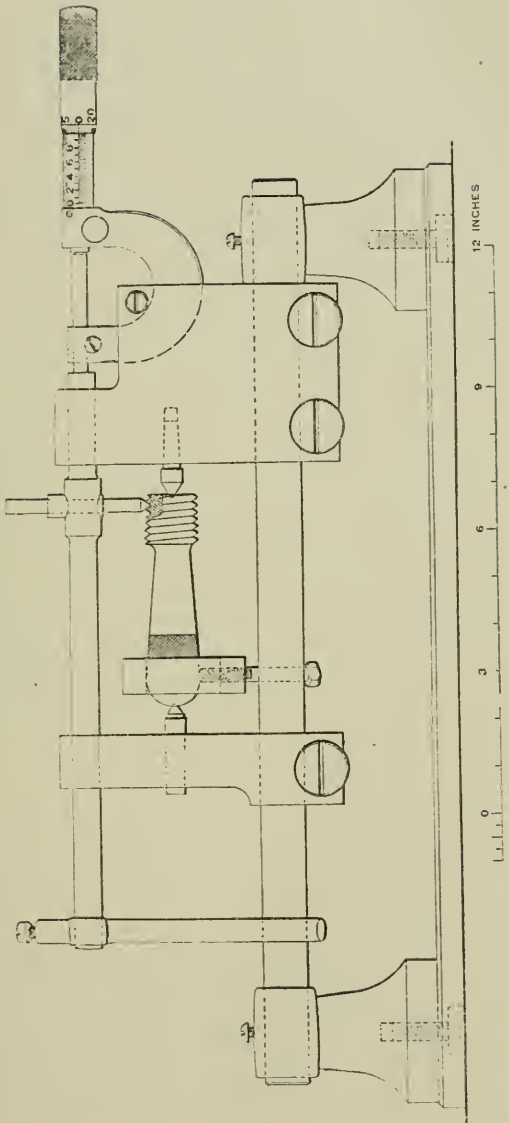


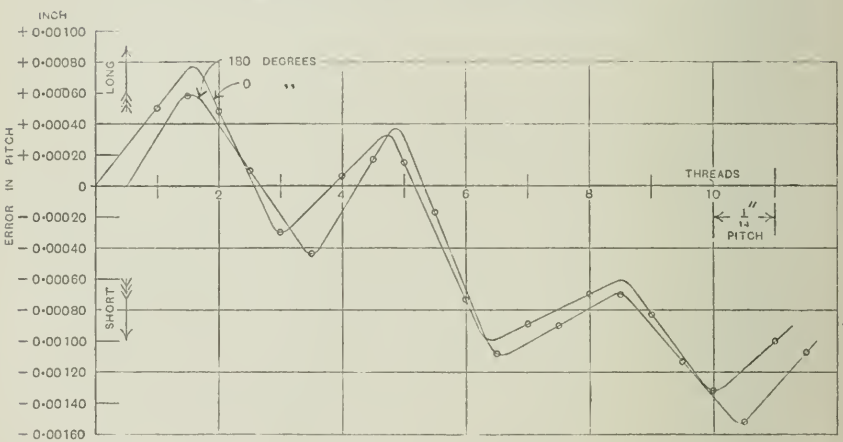
FIG. 1.
Apparatus for Pitch Measurement.

Two methods of correction for progressive error were considered:—

(*a*) By accelerating the motion of the top slide of the compound rest by means of a weighted lever attached to the screw. The screw of the slide was turned at a uniform rate as the saddle traversed along the bed of the lathe, by making the end of the lever move down an inclined plane, the slope of which could be readily determined from the known pitch error.

FIG. 2.

Tests taken on opposite sides of a test-piece of 14 *threads to the inch, leading screw-pitch* 4 *threads to the inch.*



(*b*) By selecting a new train of wheels, a process demanding some considerable calculation by trial and error for different pitches, but which has proved quite possible in all cases. For example, the train of wheels, drivers thirty-eight and twenty, followers ninety-one and fifty, gives a pitch of twenty-four to the inch correct to less than 0·000007.

The periodic errors corresponding to the revolution of the leading screw may be due to :—

(1) Reproduction of an error in the leading screw.

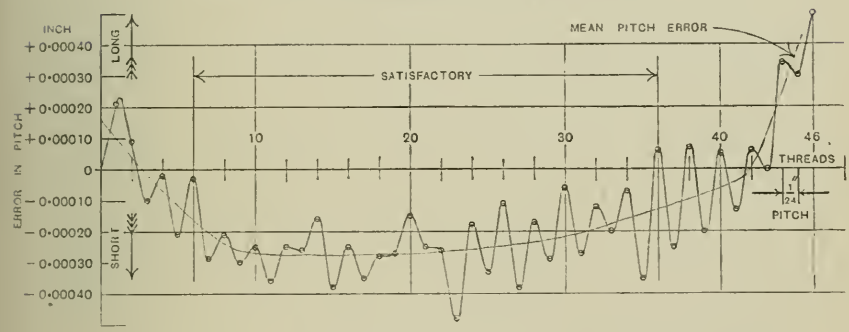
(2) An axial movement of the leading screw with each rotation, owing to both the thrust collar and bearing being out of truth.

(3) Lack of straightness or stiffness in the leading screw causing oscillations at the point of contact of the nut.

To correct (2), the collar bearing on the leading screw was dispensed with, and the thrust was carried by a single ball-bearing applied to an enlarged centre on the leading screw. The collar was turned off and the screw made to run free in its bearing. A half-inch ball was used, and the thrust taken by a bolt screwed

FIG. 3.

Final Test on a Screw showing a satisfactory range for a length of $1\frac{1}{4}$ inches.



through a bracket, and locked in position, the end of the bolt being made intensely hard.

After testing and correcting the change-wheels for eccentricity, it became evident that a more accurate and rigid leading screw was necessary. A new leading screw was cut on the milling machine on a sleeve of 2 inches diameter and 8 inches in length, with a pitch intended to be twelve threads to the inch. A split nut $2\frac{1}{4}$ inches in length was clamped by a weighted lever. Mounted on the original leading screw as shaft a progressive error of 0·0008 was found, with a periodic error amplitude nearly 0·0003. On substituting a short stiff shaft the latter was reduced to an amount

not exceeding 0·0002, which may be considered satisfactory. The progressive error was approximately corrected by the following gear-wheels:—

For 24 threads to the inch $\frac{45 \times 30}{38 \times 71}$ Drivers. Followers.

For 14 threads to the inch $\frac{36 \times 67}{74 \times 38}$ Drivers. Followers.

The final test, showing a satisfactory range for a length of $1\frac{1}{4}$ inches, is shown in Fig. 3. This length, carefully marked, was sufficient for the gauges required.

The apparatus for effective diameter measurement is shown in Fig. 4. The micrometer carriage, mounted on balls, is constrained to move in a plane at right angles to the axis of the screw to be tested. This apparatus may also be used for measuring core diameter, by using triangular prisms in place of the small cylinders.

An ordinary 0 to 2 inches micrometer, fitted with a special adjustable attachment to enable direct measurements to be made of core diameter, is shown in Fig. 5 (page 56).

Optical Projection.—Enlargement by optical projection was first resorted to for adjustment of the shape of tools for screw cutting, and verification of the shape of thread. Finding the possibilities for accuracy of measurement were greater than was thought possible, and well within tolerance allowed, the use was extended to a large variety of gauges, particularly to plate gauges of shape, slots and holes, curvature and cone angles, etc.

The key to the method is to secure, as nearly as possible, a truly parallel beam of intense light, freed from heat rays by passage through a saturated solution of alum. An optical bench of great rigidity, and sensitive and universal adjustments, carries the gauge or tool and projecting lenses. The screen is at a distance of about 16 feet.

The necessity of a parallel beam (except for tool edges) should be obvious in dealing with gauging surfaces such as plates and screw-threads, and adjustment for tangential or grazing incidence must be as fine as possible. This limits the field for simultaneous survey to that of the area of the projecting lens. A combination of

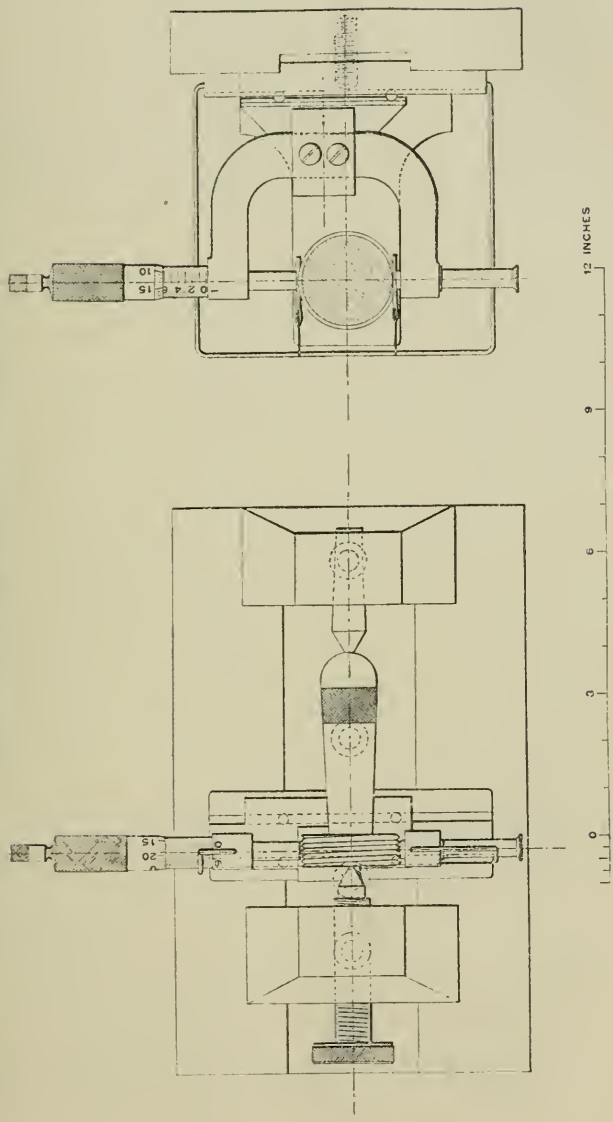


FIG. 4.
Apparatus for Effective Diameter Measurement.

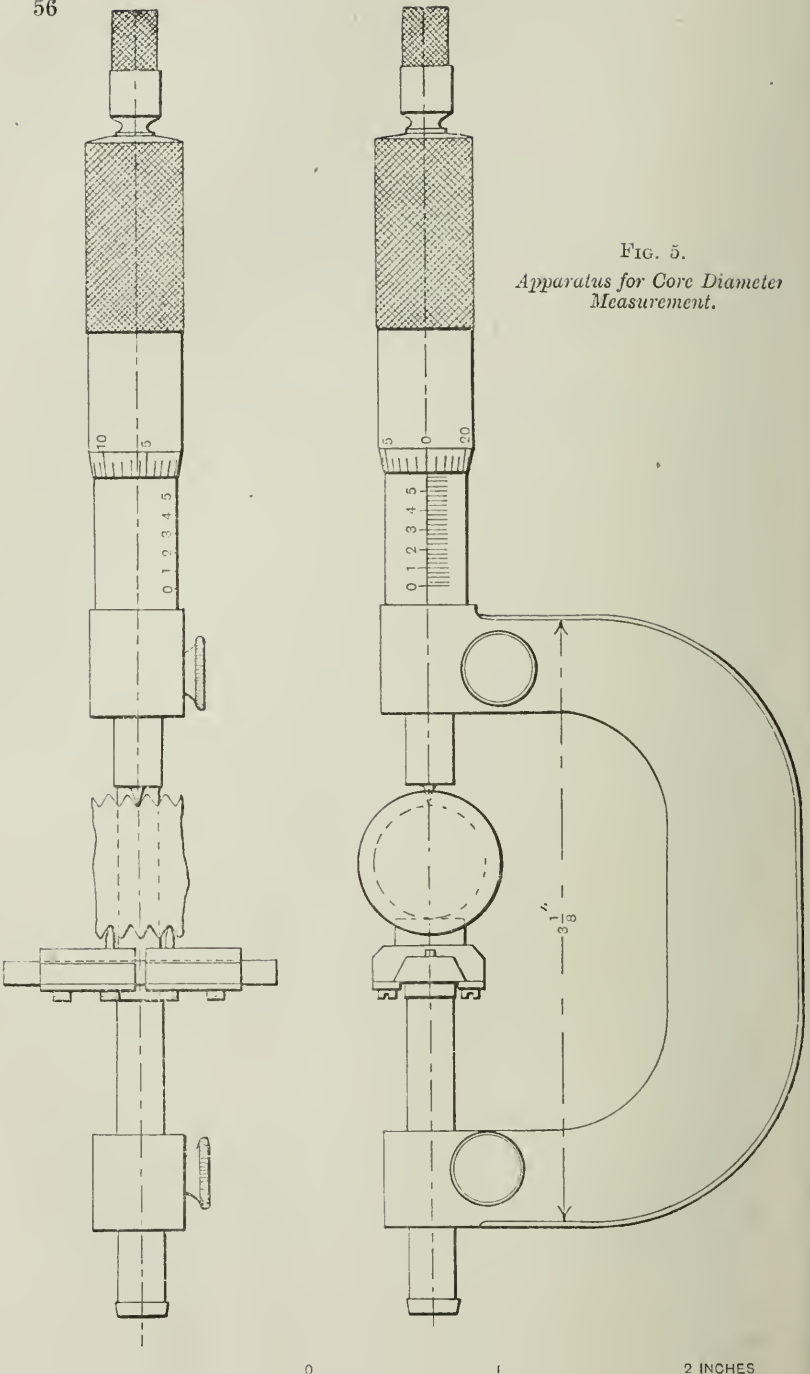


FIG. 5.
Apparatus for Core Diameter Measurement.

three optical elements was designed to give variable magnification up to 400. About 180 is however ample, and visible in undarkened room. Where it was desired, as in a few cases, to cover a larger gauge length than about half an inch, a triple achromatic lens of 2 inches diameter and 6 inches focus was combined with an astigmatic lens. The area surveyed on this extended up to over $1\frac{1}{2}$ inches, with fifty magnifications on a 6 feet screen. Quick interchange of gauges and tools from optical bench to vice or lapping motor with the minimum of readjustment is essential.

Shaping of Screw-Threads.—With a magnification of 166 a clear definition of the working edge of the single pointed tool for screw cutting is possible to much less than a ten-thousandth, this being represented by one-sixtieth on the screen. The angle and rounding off of the nose for the required pitch are adjusted with a fine oilstone to this degree of accuracy, after hardening without tempering, and quickly readjusted after wear. The same tool is used to cut the groove in a capping tool for the crests. It is then possible to insure correct shape, provided that the relation of core to effective diameter is checked by measurement.

In the final projection of the screw-thread in silhouette, a careful examination for symmetry is advisable, checking the setting of the tool. A small margin in all dimensions is left for lapping which removes some residual errors, the margin being increased if a hardened gauge is required. For rapid lapping the screw-gauge is held in a self-centring chuck on the shaft of a 1 horse-power electric motor. This is automatically controlled by a reversing switch, so that it rotates with precision, uniform torque, and any desired speed, two revolutions forward and backward.

The laps are made of cast-iron, cut with a tap, or on lathe, split, and adjustable for wear. Frequent retapping retains the shape. For the core a lap is prepared with a slightly thin thread, and for effective diameter a full thread with crest removed.

The Paper is illustrated by 5 Figs. in the letterpress.

Discussion in London.

The PRESIDENT, in moving a hearty vote of thanks to the Authors for their Paper, said the subject with which it dealt was one of enormous importance at the present time. The production of the great number of accurate gauges required in munition manufacture had been a very difficult matter to tackle, and he was afraid they were not yet at the end of all the difficulties connected with obtaining the gauges required.

The resolution of thanks was carried by acclamation.

Mr. E. M. EDEN congratulated the Authors on their Paper, which was a record of a very good piece of work done under very great difficulties. He had visited the Paddington Institute and inspected the apparatus being used, and it obviously must have been very difficult for the Authors to reach the state at which they had arrived with such material at their command. They had shown the greatest perseverance in overcoming difficulties, and were to be congratulated on having done so. He had been particularly interested in seeing the method they used for altering the change-wheels in a lathe to correct a very small error in the pitch of the leading screw. Those engaged at the National Physical Laboratory had not heard of anybody else using that particular method.

He thought it would be of interest to the makers if he showed some illustrations of the measuring machines and methods which were mentioned in the Paper, and which originated at the National Physical Laboratory. In the first place, he desired to say a few words as to the policy of the gauge-measuring staff at the Laboratory. In measuring large quantities of gauges two methods might be employed. Those gauges that were considered right might be put into one lot, and those that were considered wrong might be put into another lot, the tests being made solely by "go" and "not go" check gauges. Unfortunately

that method would not be satisfactory in the case of difficult gauges, particularly in the case of screw-gauges, because it was of no assistance to the manufacturer merely to return such a gauge to him and tell him that it was too large or too small. The manufacturer wanted to know more about it so that he could do better the next time. The method that had been adopted all through in the case of screw-gauges and difficult gauges was to measure all the different elements of every gauge. That involved a large amount of work which necessitated improved and more rapid methods of measuring, many of which were valuable to manufacturers as well as to an institution such as the National Physical Laboratory. It had always been the custom of the Laboratory to do all they could to assist manufacturers, who were welcome there; the methods used there were freely explained to them, and every assistance was forthcoming to help them over any difficulties in connexion with measurements. The various machines, apparatus, and drawings were freely offered to them to use in any way they liked.

With regard to pitch errors in lathes, the National Physical Laboratory, particularly in the early days, set out to assist manufacturers in getting their leading screws correct, and the general method adopted had been that the manufacturer, wishing to get a good screw, cut a trial screw on his lathe. That screw was then sent up to the Laboratory, which reported on its accuracy, and suggested methods of improving the lathe and reducing the pitch errors. That had been done in the case of the Paddington Institute. Figs. 2 and 3 (pages 52–53) were examples showing the sort of thing that had been done. Fig. 2 was taken from a report of the National Physical Laboratory, dated November 1915, on the pitch errors of a test-screw from the Paddington Institute, while Fig. 3 was taken from a report of the National Physical Laboratory dated 4th April 1916. The great improvement that was noticeable in comparing the two diagrams was due to the reduction of the progressive error by the use of suitable change-wheels as described in the Paper, and to the reduction of the periodic error, partly due to the fitting of a ball thrust-bearing—a standard method

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recommended by the Laboratory in such cases. Fig. 4 (page 55), representing a little apparatus for effective and core diameter measurement of screws, was introduced at the National Physical Laboratory in the autumn of 1915. The older method which had been previously employed was much more difficult.

Fig. 6 showed the method used by the National Physical Laboratory for measuring core diameters before the sliding micrometer was introduced. Three little V-pieces were used. The method looked all right in the illustration, but in practice it was difficult and required considerable skill, and it was slow.

Fig. 7 showed the old method of measuring the effective

Former Method of Measuring Core and Effective Diameters.

FIG. 6.

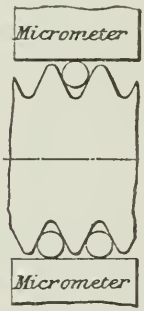
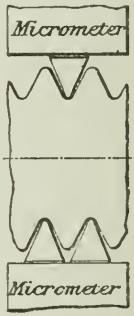


FIG. 7.

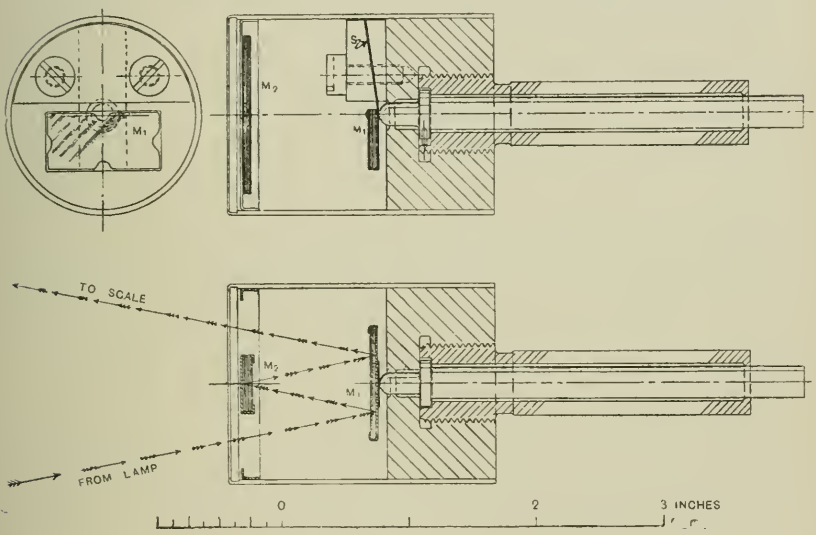


diameter with little needles or cylinders, three again being used. He wished to point out that the three V's or three-needle method was not recommended now by the National Physical Laboratory. They never used it themselves, but always preferred to have the micrometer held squarely to the screw, so that two needles or two V's only were used. The little machine of Fig. 4 (page 55) was illustrated in a National Physical Laboratory pamphlet on "Some Notes on the Production and Testing of Screw-Gauges" issued in December 1915. Fig. 8, Plate 1, represented a design of the sliding micrometer machine which had been in use for some time.

Fig. 9 showed a sliding micrometer machine fitted with a spot-of-light indicator intended to increase the accuracy and speed of

screw-gauge measurements. The light from a small two-volt flash-lamp passed through a lens and through the glass window of the indicator (*see* Fig. 10); the light was reflected on mirror M_1, then on mirror M_2, and a second time on M_1, finally passing out through the window and forming a bright image of the lamp filament on the small screen. The mirror M_1 was fastened to a thin strip of steel, S, 0·002 inch to 0·005 inch thick, which acted as a cantilever and was bent by the ball end of the plunger, the other

Fig. 10.—*Optical Indicator.* (*See* Fig. 9, Plate 1.)



end of which formed the anvil of the micrometer. With the screen about 1 foot away from the indicator, a movement of 0·001 inch of the plunger caused the image of the filament to move through 1 inch.

This form of indicator was accurate and quick to work with, and the working pressure was constant. It was particularly useful in special work, and could with advantage be applied to measuring machines. The presence of taper or out-of-roundness in a

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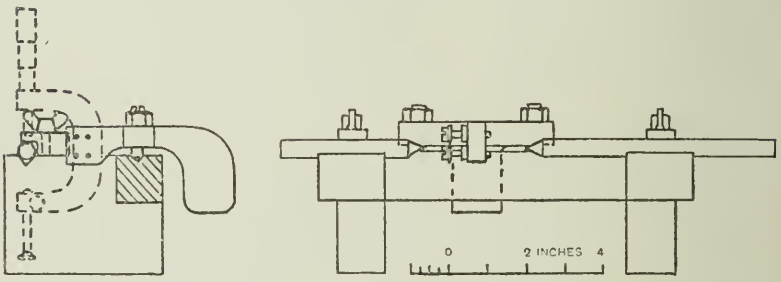
cylindrical standard was readily detected. For the ordinary work of gauge-testing the indicator was not used, as the observers at the National Physical Laboratory had become so skilled with the ordinary sliding micrometer machine that the additional complication of the indicator was not thought necessary. The Brown and Sharpe micrometer was a very accurate instrument. It was found at the National Physical Laboratory that the errors of the screw through the whole range were generally inside one ten-thousandth of an inch.

Fig. 11, Plate 1, and Fig. 12 showed another simple form of machine intended for workshop use. The micrometer tipped about

FIG. 12.—*Simple Screw Measuring Machine.*
(*See* Fig. 11, Plate 1.)



the two ball-pointed screws, which were guided in a V slot in the U-shaped casting, so that it could tip up and down and yet keep square. Exact squareness to the screw-thread was obtained by adjusting the screws. Two needles were shown in position measuring the screw, and readings were taken on the micrometer head in the ordinary way.

Fig. 13, Plate 1, showed a larger machine for measuring screws up to 6 inches which had been made to the design of the National Physical Laboratory at the Paddington Institute. It was in use, and worked very well indeed. It took screws up to 6 inches diameter, and in other respects it was of the same construction as the other machines. The large micrometer ran on balls. No screw

was shown, but the little V-pieces could be seen hanging down ready to measure a screw. There was a very light aluminium dial on the micrometer spindle, so that the divisions were easily seen. Working rapidly, particularly at night, it was an advantage to have the divisions rather widely apart. At the same time, he desired to warn the members that that was not the case if a heavy dial was put on which interfered with the feel. The inertia of a heavy wheel interfered with getting the same pressure every time. The dial used was of very thin-spun aluminium. At the National Physical Laboratory they always read the effective and core diameters directly—that is, the reading on the micrometer gave the answer, and no further arithmetic had to be done. The necessary corrections were applied beforehand setting up against the standard. In ordinary workshop use there was no need to do that. He mentioned that machine particularly because it was made by the Paddington Institute, and they were at the present time constructing another one.

The next point with which he wished to deal concerned the projection of screw-threads and gauges. The measurement of gauges by projection, as far as he knew, originated at the National Physical Laboratory about a year ago, and it had been used ever since. There was this much to be said for it—that, as soon as one knew how to do it, it was perfectly easy. On the other hand, a great many people considered it was difficult, if not impossible, to project an image of a screw on to a screen with any accuracy, on account of distortion. Fig. 14, Plate 1, is a photograph of a screw taken in the early days (it was not a very good one; better screws than that were obtained now); he showed it to illustrate what the projected image of a screw-thread looked like. In a projection machine such an image was superposed on a previously drawn diagram, Fig. 15, which was of the true Whitworth form, and it was possible to read off what the errors were. That could not be done by the older methods of examination with the microscope, and nothing like the same amount could be seen at once. To use this method, the magnification had to be fixed at some definite ratio. Hitherto they had not attempted anything very high, the standard

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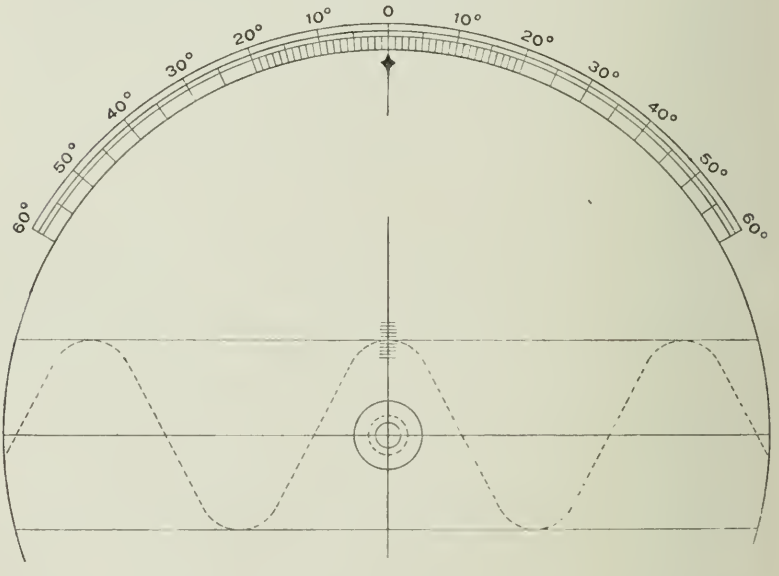
magnifications that had been used being fifty diameters. One-thousandth of an inch became one-twentieth of an inch, and it was possible to read, at any rate, to something less than half a thousandth, though one-tenth of a thousandth was not certain. Considerable difficulties were experienced when a start was made with the projection problem, because those dealing with it did not quite know what sort of lens should be used. As the Authors

FIG. 15.—*True-Thread-Form Disk, showing Screw enlarged.*
(*See* Fig. 14, Plate 1.)



had pointed out, there was no lens designed for that particular purpose.

Fig. 16 showed a design that had been made for the assistance of anybody who wanted to put up a simple-projection machine. The projection lens was shown on the diagram; there was also a little arc-lamp and a condenser which gave a beam of light facing the screw-gauge. The lens must be fixed squarely with reference to the screen. The light itself—that is, the arc-lamp and the

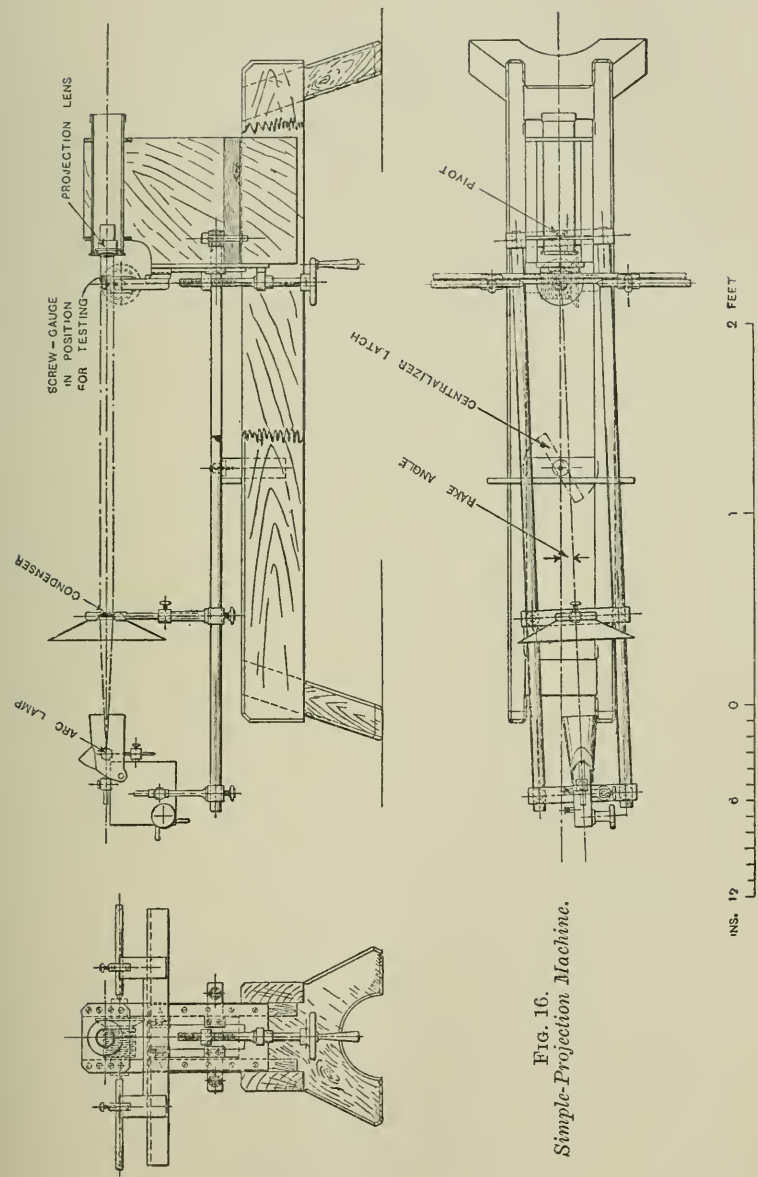


FIG. 16.
Simple-Projection Machine.

(Mr. E. M. Eden.)

condenser—had to be turned round so that the light ran along the direction of the screw-thread. The light came skewwise through the lens, and then straight away on to the screen, the lens remaining square with the screen. One of the best lenses to do that work was the one referred to by the Authors—the Dallmeyer portrait lens.

The Laboratory had experimented a good deal in endeavouring to find suitable lenses. German lenses, which in certain instances were very good indeed, were not to be obtained; the English makers had no stocks of their usual lenses, and it was impossible to order a lens to be specially made on the chance that it might be the sort of thing that was required. It was usual to get a selection of lenses and try them at the Laboratory. The National Physical Laboratory had now bought a Dallmeyer lens, but had not yet used it. The distortion problem, if the machine were used in its simple form, was not very difficult. He had not been able to do any good at all with a cheap lens, although he had made several attempts. A lens of about half-an-inch diameter would give, with a parallel beam and 50 magnification, a picture on the wall of about 2 feet 1 inch diameter, which would in general show distortion; but if it were a good lens, it would be found that there was a central portion which, as far as could be seen, was free from distortion, and this centre portion only should be used for measurements. The distortion was measured by projecting a parallel cylinder of known diameter, and seeing if it appeared, when projected on the screen, as a cylinder, or barrel-shaped, or smaller at the centre of the field than at the margin.

Fig. 17, Plate 1, showed a compound-projection machine which had been in regular use at the Laboratory for nearly a year. This machine has a complete set of thread-form disks for Whitworth and other threads similar to the diagram.

Fig. 18, Plate 2, showed the projection machine with a large field of view to which the Authors had referred. The large field of view, about 2 inches free from measurable distortion, in the original National Physical Laboratory machine, was obtained by the combination of a Ross triple binocular lens of 6-inch focus with a

6-inch Xpres photographic lens, the binocular lens acting as a field lens. This particular combination of lenses was chosen because it could be corrected for distortion by adjusting the distance between the two. This type of machine was intended for plate and form gauge work, and had been found of very great value.

Fig. 19 showed an example of the type of gauge that such a machine could deal with. Fig. 20 showed a diagram of another gauge which it was very difficult to measure except by projection. The Ministry of Munitions had had a number of these machines

Two typical Profile Gauges measured by Projection.

FIG. 19.

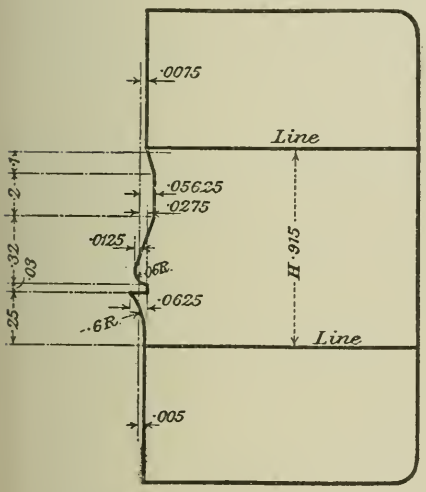
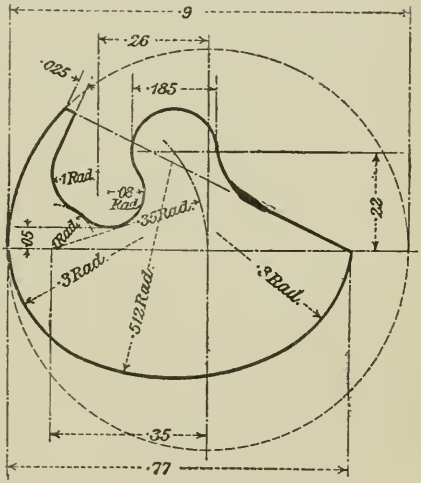


FIG. 20.



made, so that it was possible for the machines and lenses to be obtained by anyone who wished to use them.

He again desired to say, in conclusion, that the National Physical Laboratory welcomed inquiries from manufacturers who experienced any difficulties in regard to measurements. They invited manufacturers to come to the Laboratory and see the way in which they did their best to measure gauges, and the Laboratory would always at all times be ready to assist in any way they could, either in regard to measuring machines or any difficulties experienced in connexion with measurements.

Mr. W. H. BOOTH said the subject of the Paper, which was one of extreme interest, should have been dealt with twenty or thirty years ago. He had been much gratified to see the way in which men who had had nothing whatever to do with gauge-making, either of the flat order or screw-gauges, had risen to the occasion during the past two years, and he had watched the gradual increase of their skill and their perseverance in overcoming obstacles in a way that he thought had been very creditable to them. The technical institutes in London had, in particular, done exceedingly well. The Paper afforded evidence of what had been accomplished by one of them, and others had also done similar work. He knew of a little garret in London in which three men, working with insignificant little machines, had gradually progressed until they were now making accurate screw-gauges. Much of what had been done should have been unnecessary.

No man could make a screw-gauge accurately unless he had an accurate leading screw. The Paper described one method of correcting the inequalities of leading screws. Most of the members were aware of the fact that at the National Physical Laboratory there was a large leading screw which had been made for a special function, and corrected to such a degree of accuracy that it had fulfilled its purpose. Every manufacturer of machines requiring leading screws should have in his possession a leading screw which was accurate, and the main point that required attention in the near future was that leading screws should be made accurate. The National Physical Laboratory might, he thought, very well undertake that work and give guarantees of accuracy for screws which they finished for manufacturers. A manufacturer who went to the cost of having that done—and the cost need not be very great—could always sell a lathe, with what might be called a Kew certificate, in the same way that watches were sold, and he would get back all that he had spent in that manner. If something of that sort had been done twenty years ago, this country would not have been in the condition it was at the present time; they would not have had a nation of seventy millions hammering at their doors, and the country would have been in a better position to meet the

necessities of accurate mechanical construction than it was when the war commenced.

It had been his experience that those who had made a success of screw-gauges had started from the beginning by making their own single point and other necessary tools, and developing suitable jigs in which tools could be ground with accuracy to the proper thread-angle, and generally consolidating by particular labour the road they had to follow. Only in this way did it seem usually possible for most gauge-makers to arrive at that regularity and certainty of correct output that was necessary to ensure acceptance of screw-gauges by the National Physical Laboratory, in whose hands all gauges were tested for correctness.

Mr. P. A. BENTLEY inquired whether the methods of lapping used by the Authors were secret.

Mr. A. G. COOKE said that they had not referred in the Paper to the lapping except to state that it was a cast-iron lap.

Mr. BENTLEY asked if emery powder and oil were used.

Mr. COOKE replied in the affirmative.

Mr. W. G. TUNNICLIFFE said that at Paddington they generally used rouge for finishing the laps, depending on what part of the gauge required lapping, whether the crest, the core, or the effective diameter (which was the flank of thread). It was necessary to cut the lap according to which portion it was desired to lap off the screw-gauge. He did not think there was any secret about it at all.

Mr. W. BIRKINGER asked what was the correct method of lapping a plain cylindrical gauge.

Mr. P. A. BENTLEY said he was not quite clear about one point connected with the lapping process. As the Authors stated, a cast-iron lap was used; but one was at the mercy of whatever was used

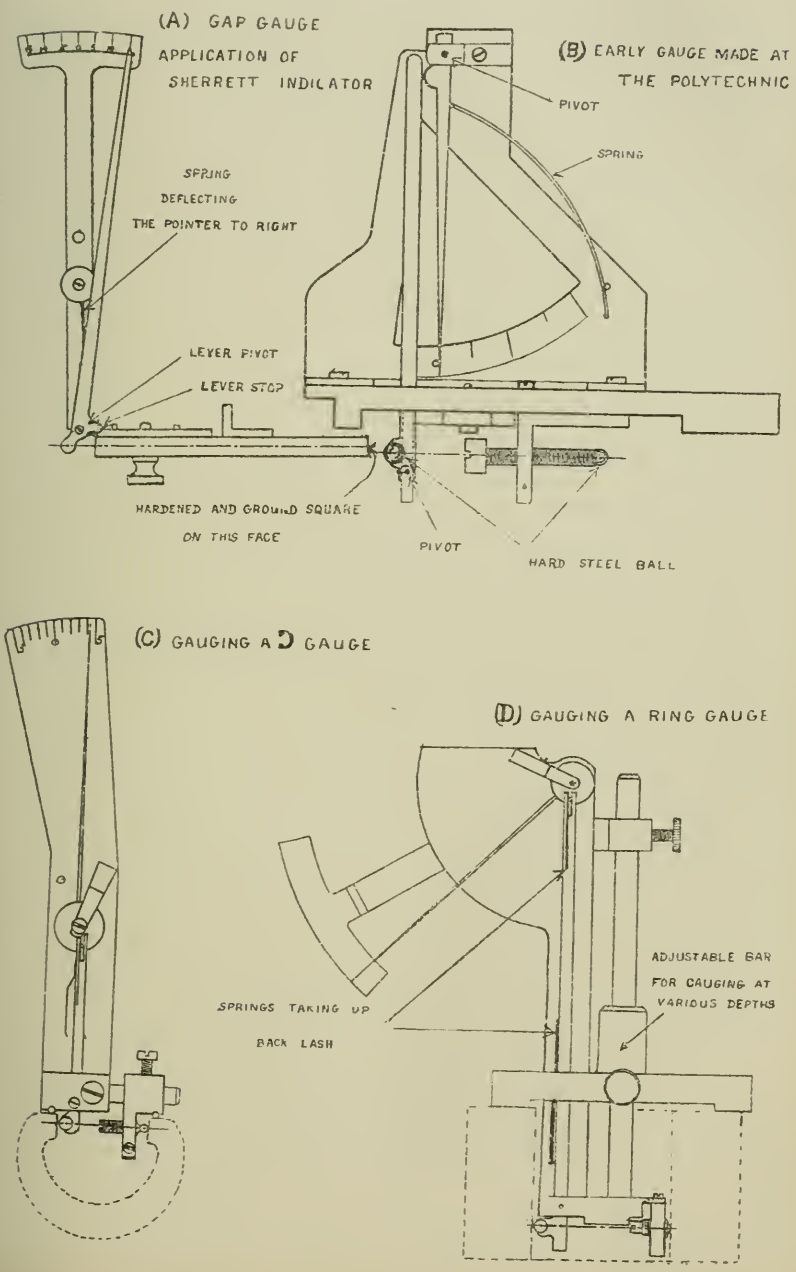
(Mr. P. A. Bentley.)

for lapping, when that operation was performed, whether the gauge was scrapped or not. He contended that there must be other methods adopted which were under supervision the whole of the time, and also under measuring supervision too. So far as his knowledge up to the present time of screw-gauge lapping was concerned, all the work seemed to be done on the same process, and to his mind that was not satisfactory. The Authors might be achieving their object, but there were certainly better methods to be adopted. For instance, Figs. 21–3, Plate 2 showed photographs of a micrometer fitted with the new attachment for workshop measuring. Any mechanic could readily understand its working, and easily determine a ten-thousandth part of an inch. The attachment comprised a large divided wheel, the readings being taken through two stadia wires, thus eliminating any parallax. Fig. 21, Plate 2, showed the attachment fitted on a lathe mounting, and Fig. 22 showed it fitted for ordinary workshop use.

Fig. 23 illustrated a lapping machine for flat surface work, its weight being about 30 cwt. It comprised a flat revolving lap, 20 inches diameter, which was driven through a worm-gear, the work being held by a magnetic revolving chuck which was raised or lowered by a rocking bar and micrometer adjustment.

Mr. H. ADE CLARK said he could not adequately discuss the Paper for the simple reason that, while the Engineering Department at the Northern Polytechnic had made gauges, they had not made screw-gauges. The Paper was confined almost entirely to screw-gauges, and the few remarks that he would make would deal with the question of principle and practice applicable to cylindrical gauges, both the plug- and ring- and tongue-gauges. He had brought to the Meeting two specimens of a little development that had been carried out by his department to facilitate the making of measurements during the actual manufacture, Figs. 24 (B) and (C). When a man was turning, grinding, and perhaps lapping an ordinary cylindrical gauge, he constantly used, if he was allowed to do so, a micrometer, with the result that he damaged the micrometer both on its table and

FIG. 24.—*Gauges used at the Northern Polytechnic.* Scale ½ size.



(Mr. H. Ade Clark.)
on the screw. It could, therefore, simply be treated as an auxiliary measuring appliance, and it was necessary to check the work closely later on. That had been avoided by his department to some extent, particularly in the manufacture of the ring-gauge, where it was difficult to use the micrometer to measure directly, and where a plug and lead-up plugs were wanted to get the size of the gauge. There they had developed the indicator rather than direct measurement. They set up a micrometer on a stand, and adjusted it to the dimension that was actually required, say, in the ring-gauge, and then used a little indicator which had a pair of ball points, and magnified the actual movement between the micrometer points. The instrument was then transferred to the work, Fig. 24 (C) and (D), and it was seen whether the indication on the indicator was the same as on the micrometer. Gradually in that way it was possible to lead up to as near the dimension as the appliance would allow one to get, and finally, if it was desired, a standard check-gauge could be used.

The indicator had been found of such use that they had done without the actual master-gauges in a very large number of cases. They recently made twenty-five cylindrical gauges of about 2 inches in diameter and about 2 inches long, which required a great deal of careful grinding and gauging. It might be thought that they would have made standard plugs to check the gauges on, but as a matter of fact not a single one was made; the checking was done entirely with a little indicator, Fig. 24 D, which was made so that they could slide it on the front. It was simply a shop-made instrument, and he did not suppose there was anything novel about it; but it struck him that it was a development that could be used profitably for a great deal of work, and it certainly saved the instrument with which it was necessary to do the actual measuring. There was a little pointer which ranged over a small scale, and a control-lever ball and a fixed ball-point, both of which were hardened steel balls. One was fixed to the gauge and the other to a lever which multiplied the motion very largely. It did not matter what the multiplication was, provided it was large, because it was only necessary to set the instrument between the two

micrometer points for the size actually required; the reading on the gauge was taken and transferred to the gauge being ground. In that way it was found that, with a little practice, it was possible to get measurements to a considerable degree of accuracy without constantly using the actual micrometer or other measuring instrument, and without going to the trouble and expense of making a large number of plugs or check-gauges or lead-up gauges. The same thing was used in a different form for gap-gauges, and it had also been tried for external gauges, though for the latter other means could be used more readily. He had a specimen of the same sort of thing, only much lighter, for a small gap-gauge or for a very small diameter. It had no extension piece, as the other one had, which enabled one to go either to the front edge of a deep hole or down to the back edge. Of the twenty-five gauges made, three were returned by the National Physical Laboratory to be adjusted; they were simply a little too small, which was all that was the matter with them; so that the method, as evidenced by that result, was of a satisfactory character. His department used a number of those gauges of various types. The original suggestion for the indicator gauge was made by Mr. G. R. Faul, shop foreman at the Northern Polytechnic, and the developments were mainly due to him and Mr. Avey, instrument maker, of the Physics Department.

Dr. WILLIAM GARNETT said that, like the previous speakers, he had been extremely interested in the manner in which men had unexpectedly come forward and risen to the occasion in the production of such very accurate work. Men who had been accustomed to work to the first order of small quantities had found themselves capable, in a very short time, of working to the second order, and some, it might almost be said, to the third order. The work which had been done by the educational institutions throughout the country had really been something to wonder at. He believed that more than 100,000 gauges had been made in educational institutions in England alone during the last eighteen months. It had afforded him much pleasure to read the Paper, because the Paddington Technical Institute had been a sort of

(Dr. William Garnett.)

child of his for a good many years, ever since the London County Council purchased the original building from a Sisterhood which erected it as a higher grade school in the belief that a higher grade school would pay a dividend upon the cost of the building. They found out their mistake and sold it to the London County Council, and the rest of its history was known to the members.

There were a few technical points connected with the Paper about which he would like to ask the Authors some questions. The first question he desired to ask was whether the apparatus exhibited for measuring the pitch of the screws would not be better if the bottom bar, to which the clamps were attached, were a rectangular bar, say $2\frac{1}{2}$ inches deep and $\frac{1}{2}$ inch wide, instead of being a circular bar. It appeared to him that the setting of the clamp by a set-screw upon a circular bar was apt to give a slight cant to the clamp, and thus put a strain upon the sliding bar, which introduced a great deal of unnecessary friction. That would be prevented if the clamps were fixed upon a rectangular bar of very considerable depth.

He had been much interested and immensely pleased with the apparatus for producing the reversing motion in the motor for lapping the screw-thread. He believed Mr. Cooke had been a cyclist from his early days, and it was therefore not surprising to find that he had adapted an important element from one industry and carried it into another, namely, the bicycle free-wheel and bicycle chain, to produce the continuous rotation of the commutator from the alternating motion of the motor. To see the electric motor make half-a-dozen turns in one direction and then in the other, quite automatically, although the chains were all running in the same direction, was at first sight rather puzzling. He had wondered whether the lapping arrangement would produce an accurate thread when the period of a periodic error was an exact turn of the thread, or when the periodic error appeared for an integral number of times in one turn. It appeared to him possible that the lapping arrangement then would simply follow the thread. But when the periodic error was a turn and a fraction of a turn, or, taking the case given by the Authors, where a screw of

14 threads to the inch was cut from a leading screw with 4 threads to the inch, so that any periodic error on the leading screw was repeated every $3\frac{1}{2}$ threads upon the screw which had been cut, then the errors were obtained upon opposite sides of the cylinder alternately. The lap, gripping, as it did, some half-dozen threads, tended to produce a screw which was the mean of all those threads, and when the errors were alternately in one direction and then in the other, the mean would be the accurate thread. But when all the errors were in the same direction upon each thread, it appeared to him that the lap might possibly follow those errors and not give any considerable increase in the accuracy of the screw.

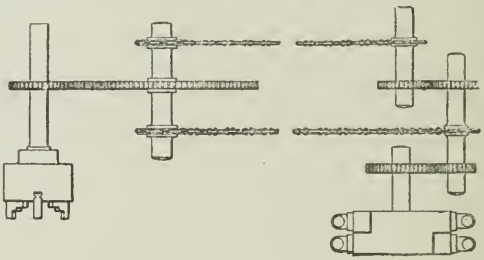
There was one point which was rather puzzling, and he would be obliged if the Authors would state whether the explanation which he ventured to offer was the correct one. The statement was made (page 47): "Hence the apparent paradox that high accuracy in the inspection gauges allows more laxity, and so increases facility and speed of manufacture." It was a little puzzling at first and quite paradoxical, that high accuracy in the inspection gauges could allow a greater amount of freedom in the manufacture of the article. He supposed the meaning was that, inasmuch as the tolerance on an inspection gauge was always outwards, plus on the H and minus on the L, the high accuracy that the Authors referred to afforded the assurance that the tolerance should be always outwards, so that the manufacturer might be perfectly certain that if his work passed his workshop gauges (in which the errors should be always inwards), the inspection gauges would allow a larger margin of error on the work than his workshop gauges, and consequently he was sure that his work would pass. In fact, a larger error was allowed in proportion to the amount of tolerance upon the inspection gauges, whereas if the inspection gauges were made inaccurate in the sense that the tolerance was inwards instead of outwards, then there would be no such certainty. Possibly that was the explanation of the words in the Paper which, when he first read them, rather puzzled him.

Mr. A. G. COOKE, in reply, said that he did not quite mean to

(Mr. A. G. Cooke.)

interpret the apparent paradox that was referred to at the bottom of page 47, to which Dr. Garnett had referred, in the manner that the speaker had suggested. He meant that if it were possible to obtain gauges in sufficient quantities of a very much higher order of accuracy—that is, to reduce the tolerance to one ten-thousandth instead of three ten-thousandths—it would be possible to fix the values, so as to leave a wider margin inside the limit. It would not be the accuracy of any individual who was engaged on the work ; but if accuracy were possible in all work, if they could all work at one ten-thousandth of an inch instead of three ten-thousandths, a good deal more margin could be left to the manufacturer who had to work within those limits. It was like putting a narrow border

FIG. 25.—*Screw Lapping Motor Control.*



upon an area and leaving more space inside. That was what he meant to convey by saying that it was an apparent paradox.

The question that Dr. Garnett had asked with regard to the lapping and the integral number of turns was a problem he would like to consider more fully and not answer straight off. His idea in connexion with the lap was to get a high speed, Fig. 25. Working with 300 or 400 revolutions on the motor meant a good speed, and he hoped to increase it by means of improved construction. It seemed natural that a high speed should be required, and also that the period should be short. It would certainly be better if the relation between the screw-threads and the gauge and the number of the leading screw were not a simple proportion—that is, it was not well altogether to cut a screw-thread 24 to the inch if the leading screw was 12 ; it would be much better to have a leading screw of 14.

With regard to the question of gauges to which Dr. Garnett referred, he did not think he (Mr. Cooke) made it quite clear that the part of the work that the Authors thought was most successful was due to working the lapping in connexion with the high-power enlargement by optical projection. They were able to get 163 magnifications right alongside: it was a workshop and a laboratory at the same time. The whole of the projection took place on a table, and they did not mind the vibration at all. The gauge was put in the chuck; in a moment it was lifted off and transferred to the optical bench so that the shape was projected upon the screen; three laps were used, one for rubbing the crest, another for the flanks, and another for the core, according as each required adjustment. They were controlled by a direct measurement of the diameter. Mr. Birkinger had asked a question about plain cylindrical gauges, to which Mr. Tunnicliffe would reply.

Mr. Booth had referred to the question of accurate leading screws. Precision lathes were very much wanted, but the method that he (Mr. Cooke) had adopted overcame a good many difficulties when the screw-gauge was hardened. Even if one had an actual precision lathe and could cut the screw absolutely correctly, when it was hardened, unless a great improvement upon present methods known were used, certain distortions were bound to occur. The lapping which would work equally well on the hard screws ought to receive a great deal of consideration. He only threw that out as a suggestion, because it would entail an elaborate argument to enter into the question of the hardening of the screw-gauges. He had hoped that some one would have mentioned the point in the course of the discussion. The finishing operation in the lapping works was easier with the harder screw than with a soft one.

Mr. Eden had been able to add a great deal of valuable information to that contained in the Paper. He wished to give the National Physical Laboratory credit for all they had done, but he thought Mr. Eden might take notice of the projection apparatus with the three lenses, Fig. 26 (page 78). It was a very great improvement on what had previously been used. The combination was equivalent to a single lens of less than $\frac{1}{2}$-inch focal length, but

(Mr. A. G. Cooke.)

gave critical definition and high covering power. It was really equivalent to a microscope in which a cinema projection lens (Petzval formula) formed the objective, and a wide angle combination corresponded to the eye-piece. The latter consisted of a single lens of 2-inch diameter combined with an astigmatic lens. It had an independent use for projecting with moderate magnifications (up to 50) considerable lengths of gauge, particularly flat gauges of shape up to 2 inches, which were placed close behind the large lens. For this purpose it was devised, and was used, by the National Physical Laboratory. Its adoption as eye-piece in the combined system in place of a single lens enabled a much larger field to be covered. The reduction of the distance of the screen to 5 feet was not only convenient, and enabled vibration to be eliminated, but greatly reduced diffraction difficulties in

Fig. 26.—*Diagram of Arrangement to overcome the Difficulties attendant upon Projection.*

producing a perfect shadow or silhouette. It was not merely a question of focussing. With 166 magnifications a ten-thousandth of an inch was enlarged to a sixty-fourth, and the silhouette was as distinct as the eye could observe. It must be remembered that adjustment by lapping to a fine outline on the screen was to be made by a mechanic and not a trained observer, and the greater enlargement was much to be preferred.

Mr. W. G. Tunnicliffe, in reply to the question asked by Mr. Birkinger in regard to lapping, said there might be other methods in existence for cutting the screw when the steel was soft; in that case a tool full form was used for cutting correctly the core diameter and the effective diameter. But when it came to a question of a hardened screw-gauge it was a very difficult matter.

The effective diameter would probably come up in size, the pitch generally lengthened, and the screw diameter might come up a small amount; it might come up outside the limits. It was perhaps necessary to lap two ten-thousandths or three ten-thousandths off the effective diameter, and one ten-thousandth off the core on the hardened screw-gauge, and he did not know of any other method at the present time except lapping for a gauge of that description. If the gauge was 0·26-inch diameter, 20 threads to the inch, he did not think it was quite possible to grind that gauge down to the correct dimensions for the severe test which the National Physical Laboratory generally gave the gauges. They had not lapped many of the soft screw-gauges at the Paddington Institute but they were making hardened screw-gauges. Hardened screw-gauges, especially standard screw-gauges, were a very tough proposition in view of the fact that the limit was about three ten-thousandths of an inch maximum. He did not think there was any difficulty in the lapping of a plain cylinder, especially if the cylinder was ground after it had been hardened. At the Paddington Institute either a copper lap or a lead lap was generally used. It was found that a lead lap held more abrasive than a copper lap, and they closed the lap up as it wore down by means of a screw, the lap being split. In the case of a lap for a ring-gauge, the adjustment was done by means of a taper pin or screws placed at the end of the lap.

The laps used were made of close-grained cast-iron, and if the lap were required for a plug screw-gauge, which was large on effective diameter, a tap was made so that the thread which it cut in the lap was full on effective diameter, the core being well cleared. A lap thus made would reduce the effective diameter, and also eliminate any periodic error which might be present. If the core only required lapping down, a lap would be used which was small on effective diameter and the core large. For lapping a ring screw-gauge, the lap was cut up in the lathe and the part on the lap was left full, corresponding to the part on the gauge which was to be reduced.

Mr. W. J. Gow, in reply, said he desired to refer to one point

(Mr. W. J. Gow.)

which had been raised by Mr. Eden. The members might get the impression from what Mr. Eden said, that the correction of periodic error in the lathe referred to in the Paper was due to the introduction of a ball-bearing to the end of the leading screw. It was found that trial screws, cut before and after the ball was applied to the end of the leading screw, showed exactly the same amount of periodic error. The cause which was finally proved for the periodic error was that referred to in (3) on page 53. After a new leading screw had been produced, it was found that there was a certain amount of whirling of the leading screw, which was attributed to the "lack of straightness or stiffness in the leading screw causing oscillations at the point of contact of the nut." It was found, after a short stiff shaft was introduced, on which was mounted the leading screw cut on the milling machine, that a periodic error was obtained within the limits.

With regard to the hardening of gauges, it had been suggested that, while the result of hardening was to increase the pitch by, say, eight ten-thousandths of an inch, it was quite easy to allow for it by cutting a screw which was short in pitch by this amount, and provided it was possible to apply to it exactly the same heat treatment as had been applied to the trial screw, a hardened gauge of correct pitch should in the end be obtained. The problem in connexion with hardened gauges was not quite so simple as was suggested by that example. It was found that in hardening screw-gauges the diameter at one part of the screw might become very much greater than it was at another part, while in some instances parts would become smaller.

Mr. Tunnicliffe had recently carried out a very interesting experiment on the hardening of an ordinary screw-gauge with a handle at one end, the diameter of the handle being approximately half the size of the screw, the other end of the screw being plain. It was found that, without taking any special precautions, the threads at the ends of the gauge were larger in diameter after hardening, and the diameter in the centre of the gauge was smaller. In one particular gauge the diameter of the screw at the plain end of the gauge increased in size by ten-thousandths of an inch; the

diameter of the screw next to the handle end increased in size by seven-thousandths of an inch, and the diameter in the centre of the screw went down by three-thousandths of an inch. The conclusion was come to that, in regard to the experiment, the extra cooling surface supplied to the outside threads by the ends of the gauge probably caused a greater rate of cooling at the temperature of recalescence of the steel than was obtained near the centre of the gauge. In order to test that point, another gauge was hardened, after covering up the ends of the gauge so as to prevent them coming in contact with the cooling medium, and it was found that the diameters of the screw-threads for the whole length of the gauge were practically constant. He did not think, however, that this altogether explained the reason why larger diameters should be obtained at the end and a contraction in the centre. Personally, he thought it had something to do with the internal strains set up due to hardening. The Paddington Institute had produced a number of satisfactory hardened screw-gauges, and although there was still much to be done with that part of the work, those connected with it felt that, if the time should come when they were ordered to produce only hardened gauges, they would be able to overcome the difficulties.

Discussion in Leeds.

Mr. J. HARTLEY WICKSTEED, *Past-President*, who presided, before calling upon Mr. Cooke to present the Paper, said the parent Institution in London had invited the country members to hear the Papers that were read in London, and he had had many of those Papers sent to him. He had chosen that Paper on the making of Gauges as one that would be very valuable for them to hear,

(Mr. J. Hartley Wicksteed.)
and the Institution had generously taken upon itself to pay the whole of the expense. It occurred to him that the making of gauges was one of the questions of the future; it was one of the things by which they could gain from the experiences and necessities of the war. When he was in America about the year 1872 and visited the works of Messrs. Brown and Sharpe, Mr. Brown told him that the material wealth of America had been increased by their war in spite of all the sad loss of life, which they could not, of course, replace. They had, he said, been driven by the necessities of the country to make small-arms and munitions in quick time, and into developing the system of interchangeable parts—a system which would be described by Mr. Cooke, who had achieved great success, having made thousands of gauges which had been tested and approved. Mr. Brown also told him that when the war was over they found they had got into the way of working with gauges and making parts absolutely interchangeable, and that they had adopted new methods which, without the stimulus of war, they would not have worried their minds about. He might just add that Mr. Brown showed him a sewing-machine of the same kind as he (the speaker) had had in his own family for over fifty years, and which was working as well to-day as ever it did. Mr. Brown said it was no longer the best-designed sewing-machine obtainable, but it was as great a favourite as ever on account of its reputation for perfection of workmanship. He thought Mr. Cooke would be able to describe some very successful methods for attaining similar perfection.

Mr. A. G. COOKE then presented the Paper in abstract.

Mr. HENRY MCLAREN, who opened the discussion, said he had listened with very great interest to all that Mr. Cooke had put before them. Most of those present, doubtless, were concerned with the practical side. It was the lapping that interested him. They all had hardened screws to get down, and, if he understood Mr. Cooke rightly, of the three laps, one only bore on the flanks and was free at the top and bottom of the screw, while the

next one only bore on the top, and was free at the flanks and bottom, and the third one bore at the bottom only. [Mr. COOKE: Yes.] The laps would have, he supposed, to be made with special taps, for an ordinary tap might bear equally all over, causing grinding where grinding was not wanted. He would like to know about the manufacture of the laps; it was a rather interesting point to them in Leeds, where they dealt with the practical side of things. The method described in the Paper was a very interesting way of getting a correct thread, which there was nothing more difficult to gauge. They might have a thing that just went nicely down, but might be touching on a point only, giving the "feel" of a good fit, but in reality being far short of a fit. The method described was an admirable way of testing screws.

[Replying to a question as to how long the cast-iron laps lasted, Mr. COOKE said that after doing one or two gauges they generally ran the tap through again.]

Mr. MCLAREN referred to the mechanical lapping device, of which a diagram was shown by the Author, Fig. 25 (page 76), and said that he understood the free-wheel action of the gear, but would like to know how the rapid reversal of the gear was effected without causing undue stresses. He appreciated the fact that the gear was of light construction, but he would like to know how many reversals could be obtained per second. [Mr. COOKE: Four or five.]

The CHAIRMAN remarked that with a big planing machine they could get 60 reversals a minute in spite of the weights moved and the high speed of traverse.

Mr. COOKE said that the motor—when the armature was short-circuited in reversing—acted as an electric brake, and, in fact, the electric reversals of the lapping motor were analogous to the cushioned reciprocations of the piston of an engine. With wide carbon brushes on the commutator the reversals were effected very satisfactorily and without damage to the commutator by sparking.

(Mr. A. G. Cooke.)

The free-wheel gear did not carry the power, but only carried the electric control. With regard to the taps for cutting the laps, it was necessary to make three special taps, each being relieved where required.

Mr. J. W. JUKES said he did not quite understand the effect of distortion from the lens in the method of projection that had been described. It must be a very perfect lens if there were no distortion on the shadow that was thrown.

Mr. COOKE said a "cinema" portrait lens was used next to the gauge, which was almost absolute perfection. With regard to the other lens—combination—any slight distortion could be eliminated by altering the distance between the two lenses. Then it was perfectly easy to observe distortion by projecting a true cylinder and testing the image.

Professor G. F. CHARNOCK, of Bradford, congratulated the Authors upon their excellent Paper, and incidentally upon having done something to justify technical education. He thought very fine work had been done with quite ordinary tackle, and it should serve to show engineering employers that there was something worth patronizing in technical institutions. There could be no doubt that, after the war, people, having had some experience of working to gauge, would be disposed to continue that method of manufacture. It had fallen to his lot to inspect and test many machines, dealing with firms of high repute, and he had found that the system of working to gauge had not been resorted to in many, otherwise good, shops. The number of gauges one found in general use was very small indeed. Since the war began, however, in making various things for Government, people had been compelled to work to gauges, and they had been so surprised at the improvement in workmanship and in cheapening production which resulted, that after the war the same system would be adopted for ordinary work. Whether, however, it would pay firms to take up the manufacture of their own gauges was questionable; it would

probably have to be done as a speciality. But to formulate their own requirements, engineers ought to understand how gauges were made, and to be fully acquainted with such methods as the Authors had placed before them.

The optical method of measurement was a most important one, and he thought that a fuller description of the method, with a more detailed account of the apparatus, and also of the very ingenious method of lapping, ought to be included in the Proceedings. Papers such as this must prove of great advantage to all in the provinces, and he sincerely hoped that it would not by any means be the last one to be presented in Leeds.

Mr. FREDERICK GROVER drew attention to the statement in the Paper (page 47), namely, "the apparent paradox that high accuracy in the inspection gauges allows more laxity, and so increases facility and speed of manufacture." In this connexion he showed by means of a diagram how the manufacturer's latitude might be seriously curtailed by the inaccuracy and variation of inspection gauges, and for this reason it was obviously of great importance that engineers should support in every way the great work the Authors were doing in the making of accurate gauges. Many engineers had to resort to the process of thread milling, and it was known that threads produced in this way were not absolutely accurate in form. The question then arose as to whether the extreme accuracy of the inspection gauges was not—in this particular—altogether abandoned in practice. Mr. Cooke had stated that he had improved the lathe on which he cut his inspection-gauge threads by making a new leading screw on a milling machine, and he (Mr. Grover) asked whether he used the milling machine as a lathe, or milled the thread in the usual way it was done on a milling machine; as if so, it would be subject to the inaccuracy of form already noted by the speaker.

Mr. COOKE said that they used the milling machine practically as a lathe, and chose to do so because the screw for giving the pitch traverse was found to be the best they had available.

Mr. GROVER thought the information was important, as there might otherwise be some misapprehension.

Mr. WILSON HARTNELL said that most manufacturers could not make their own gauges, and he thought far too much trouble was given to manufacturers by Government inspectors on the question of screw-threads. Where, he asked, were manufacturers to obtain such accurate taps as had been described? Government inspectors could not furnish them. Manufacturers had had many things rejected because of superfluous accuracy demanded in articles which, for practical purposes, satisfied all that was required of them. Gauges were important, of course, enabling them to work within limits. If the tolerance were very small, the temperature needed to be carefully considered. They were much indebted to the Authors of the Paper for great accuracy in measurements of standard gauges for screws.

Mr. CHARLES H. CRABTREE said, as one who had been greatly interested in both the use and manufacture of gauges during the last twenty-one months, that the quality of the gauges which they now had reflected very great credit on such institutions as Mr. Cooke had mentioned. Like most firms suddenly called upon to make a big output of repetition work, his firm found great difficulty in the manufacture of gauges. They had considered hitherto that they could do what might be called fairly accurate work, but sometimes after turning out a nice job they received a Government report showing what seemed to be a large error—perhaps it amounted to eight or nine ten-thousandths. They had to put this right by making special taps and gauges, and they found the greatest difficulty with female gauges. With regard to Mr. Wilson Hartnell's remarks, he (the speaker) believed that nearly every firm in the country had suffered in the way described. Gauges, sometimes, were put into the hands of inspectors who were incapable of properly interpreting them.

Mr. G. MURRAY said that what interested him most in the Paper was the lapping. He elicited by questioning Mr. Cooke

that the shape of the lap, which was held lightly between the fingers, determined where it would touch, and that a little rouge and turpentine were used. He wished to inquire if heat was generated during the process of lapping.

Mr. Cooke replied that he did not experience any trouble due to expansion. The effect of heat on the shape of the threads would be infinitesimal. On diameters the allowance would be the same as in lapping a plain cylinder.

The Chairman moved that a vote of thanks be sent to the President of the Institution for giving them that opportunity of hearing Mr. Cooke, and their best thanks also to the Authors for the admirable way in which they had put the subject before them.

This was seconded by Mr. Wilson Hartnell and supported, on behalf of the visitors, by Mr. Pullan, and carried unanimously; and in replying, Mr. Cooke said his great object was to enhance the credit of technical education. It was a great pleasure for him to come into the provinces, and they had had a very interesting discussion.

Discussion in Birmingham.

The Chairman (Mr. Walter Deakin) said that this was the second meeting in Birmingham which had been held to continue the discussion on a Paper, and he was pleased to notice so large an attendance, notwithstanding that many of their members were so exceedingly busy upon munitions manufacture. It was unnecessary for him to enforce, at such a national engineering juncture as the present, the supreme importance attaching to gauge manufacture,

(Mr. Walter Deakin.)

bearing in mind the class of operations upon which the nation was now—he had nearly said almost exclusively—engaged. The Paper dealt with the work which was being performed by the London County Council Technical Institute at Paddington in connexion with gauge manufacture and some other closely allied engineering matters, and was a fascinating one. Not only so, it reflected the highest praise upon the London County Council and upon the teachers under them who had the work of instruction in hand. Mr. Cooke was at the forefront in this work of teaching, and this fact would add materially to the interest with which he would be listened to. The London County Council was proud of the fact that the instruction which they were to-day able to impart at their technical classes was of the highest practical value, as well as being founded upon a sound scientific basis, and certainly the statements contained in the Paper respecting what was now being effected concerning the out-turn of testing instruments of the highest magnitude, to be employed in the engineering shops in various parts of the country responsible for the national output of munitions, seemed to support the Council's action in a remarkable manner.

Mr. A. G. COOKE then gave a short description of the work dealt with in the Paper.

The CHAIRMAN said that the Paper was an important and an interesting one, the more so since Mr. Cooke had fairly established his claim that the technical work which the London County Council was doing at the present time in its instruction workshops was clearly proving of valuable assistance to the everyday operations in urgent munition manufacture of the hundreds of State and joint-stock and private-owned engineering works thickly established all over the kingdom, not forgetting by any means the West of Scotland. The bromide paper which Mr. Cooke had exhibited to the members was alone sufficient to provide food for discussion, and he hoped that, in the remarks which might be made, Mr. Cooke would collect some practical hints which it would be worth while his taking back to his technical workshops.

Mr. H. Thompson inquired what was the method for holding the lap to correct the gauge exhibited. The reason he put the question was that the lap [exhibited] being rather heavy, together with the speed of motor used, would run up against the shoulder of the gauge or run off the plain screw-gauge, which would be very dangerous to the operator. He thought the best method for testing screw ring-gauges was by means of a master plug—a plug with the crest diameter taken off to test the effective diameter.

He had been engaged in the production of gauges, and had generally used high-carbon steel. Mild steel did not appear to him to be of much value for the purpose. The life of a soft steel or unhardened gauge must always be uncertain, and often very short; it might be spoilt in a few minutes of shop use. His method was to cut up a bar of steel and anneal the pieces cut off, then turn up the pieces of steel and re-anneal to dull red, 650° to 700° C., and quench in oil, which then should be sufficiently soft enough to turn up and finish the gauge. The annealing was important. A cast-iron pot should be used of such dimensions as to suit the work in hand. Put the pot in a muffle or gas-stove containing sufficient cyanide of potassium to cover the gauge. Heat the pot up to about 800° C.; at the same time slowly heat up the gauge to 800° C. (dull red). Then immerse the gauge in the molten cyanide, seeing that the gauge was well covered, or as much of the gauge that was required to be hardened. Then the gauge should be withdrawn, and re-heated again in open gas-jets so as to burn in the cyanide. The process of immersing in the molten cyanide and burning in should be repeated at least three times. This was followed by quenching in oil, and then the gauge should be cleaned up, which should then be found to have a good wear-resisting surface, with its former accuracy retained. It was advisable to add a little cyanide to the pot each time a fresh lot of gauges was hardened, and it was preferable to burn in in open gas-jets, as the gauges could be handled easier and without fear of damage. This method was adopted to minimize lapping. During the last six months he had resorted to the ordinary methods of hardening and lapping up.

Mr. W. TURTON inquired what class of steel would the Author suggest for giving the least distortion in the manufacture of gauges and other measuring instruments.

Mr. COOKE stated that the Institute employed both cast steel and double shear steel, but generally the former. Unfortunately, however—and here came in a great deal of unnecessary trouble for the Institute—the material which was supplied differed a great deal in quality, and to avoid error they were absolutely bound to test every consignment. If the Institute could control the supplies from which the material was drawn, this consumption of time might be avoided, but at present it was being found impossible to limit the makers. This circumstance constituted one of the difficulties under which output in the workshops was being conducted to-day, and, desire otherwise as they might, it was found to be impracticable to dispense with the testing for quality of every delivery of metal. He could not say at present definitely which class of material gave the least distortion. It seemed to him a most desirable matter to see that no gauge should be sent out unless some degree of hardening had first been applied.

Mr. P. J. WORSLEY, Jun., regretted that he had not gathered from the Paper whether the gauges were finished soft as well as hard; and he would like to know whether the lapping was used for the former as well as for the latter. He also inquired whether the friction set up by lapping had not been found to be inconvenient.

Mr. H. L. HEATHCOTE asked whether it was the Authors' practice to measure gauges before hardening, and, if so, what allowance was made for alteration during hardening. The advantage of the hardened screw-gauge was, of course, obvious. In his experience, one hardened screw-gauge would outlast at least twenty soft ones.

He would like to ask whether the Authors had tried checking the pitch of screw-gauges by optical projection. He (Mr. Heathcote) had used an optical projection system not quite so elaborate

as that described by the Authors. It consisted of a single lens (Zeiss Planar, 75 mm.) which projected an image of the screw-thread on to a sheet of plain glass against which an outline drawn on tracing paper of a Whitworth thread was held. The magnification of both gauge and outline was about 20 diameters. This arrangement possessed the advantage that the observer's head did not come between the gauge and the screen; it gave quite sharp definition, and permitted the use of a magnifier by which a difference between the gauge and the outline as small as 0·0002 to 0·0003 inch could be seen quite readily.

Mr. H. C. ARMITAGE asked whether, in their experience with the general class of lathes turned out by machine-tool makers, the Institute found much error in the pitch of the leading screw; also what steps they took to correct such errors.

Mr. A. JOHNSON was interested in Mr. Cooke's description of the optical projection process adopted for measurement purposes by the London County Council, and would like to know what he employed for light-production purposes. He believed the methods adopted in this country and used in the United States Navy for obtaining a greater intensity of light highly recommended themselves. In these lamps the carbon electrodes were rotated at a constant speed by electric motors, the necessary reduction in speed being obtained by worm-gearing, while a solenoid-operated pump forced a low flash-point spirit through nozzles on to the tips of the electrodes, thereby cooling the outer surfaces, localizing the arc itself to a smaller area of greater intensity, and consequently reducing the angle of divergence of the beam to about one half.

As he had not had the opportunity of witnessing any measurements carried out by the methods indicated by Mr. Cooke, he was not quite clear as to the position in which the alum bath was fixed. If this were in such a place that the parallel beam of light passed through it, then the thickness of the walls of the containing vessel would probably tend to distort the beam; of course, if the vessel were not placed in the parallel beam, then any distortion

(Mr. A. Johnson.)
that occurred would not affect the result. He gathered from the Paper that the light rays were required and not the heat rays, these latter being effectually cut off by the alum bath. If there were any chance of the operator working for lengthy periods in the glare of the arc, it would be beneficial to adopt some means of protection from the effect of the ultra-violet rays.

He understood Mr. Cooke to say that the screw-gauges were held in a self-centring chuck for rapid lapping of the thread. It was quite possible that the gripping action of the jaws would deface the gauge. To obviate this, a magnetic chuck might with advantage be fitted.

It was noticed from the diagram that an automatic commutator type of switch was used for reversing the motor. Did Mr. Cooke experience any trouble due to sparking at the brushes at the instant of reversal? Since the circuit broken would be carrying about 4 amps at 220 volts, would there be sufficient sparking to warrant the use of magnetic blow-outs, or were low-resistance brushes fitted?

Mr. H. I. PEDLEY sought instruction on the methods which were used for testing the effective diameter of the thread of gauges during the operation of overlapping. So far, the remarks of the speakers had been confined to plug-gauges; but he was curious to know what tests were applied in the case of ring-gauges to see how the lapping was proceeding.

Mr. H. L. HEATHCOTE asked permission to reply to a criticism from one of the speakers. The tracing paper employed to show up the image of the screw-thread was not squared, but plain. To make sure that the magnification of the gauge was exactly the same as that of the outline drawn on the tracing paper, it was their practice to throw upon the screen of the "micro-camera" the image of a screw-thread the pitch of which had been measured by the National Physical Laboratory, and then to adjust the length of the camera until the accurate part of the thread coincided with the outline. Once the micro-camera had been set, the pitch-errors of

gauges could be rapidly seen and measured approximately if desired.

He would like to know whether Mr. Cooke had tried mild steel case-hardened for screw-gauges, as his firm had obtained very good results from its use.

Mr. W. Turton remarked that when he employed mild steel for gauge manufacture and such-like work he usually double-annealed and carbonized it; but it all depended upon the precise quality of the steel they were using up, and they generally found that they could not leave too much for lapping. On the whole, he thought that case-hardened mild steel was better for cutting purposes than even cast steel.

Mr. John Nasmith said that the firm with which he was connected had been for the last two years engaged in making screw-gauges which had to be passed by the National Physical Laboratory. After a considerable amount of experimenting, they had discovered and developed means whereby gauges to the required limits could be produced with accuracy and certainty—in fact, during the past few days they had received reports from the National Physical Laboratory of twelve gauges, nine of which had been passed as "reference" gauges and three as "working" gauges. All these gauges had been made out of mild stee case-hardened.

They had found out that in the making of these gauges it was most essential—first, to have correct tools and appliances; and, secondly, to lay out the operations correctly, and to see that each operation was correctly performed before passing on to the next. It had been found to be waste of time to try to overtake a mistake in an operation when once that operation was passed.

In his opinion, there were four essentials necessary for accurate production of these screw-gauges on anything like a commercial basis: (1) accurate lathes; (2) accurate cutting tools; (3) correct sequence of operations on the gauges; and (4) suitable measuring appliances.

(Mr. John Nasmith.)

With regard to the first, his firm cut threads from several of the best lathes in the shop, and submitted them to the National Physical Laboratory, and it was found that certain lathes produced screws accurate as regards the pitch within the allowable limits, but others did not. These other lathes were carefully overhauled, abutments adjusted, new and accurately cut change-wheels introduced where necessary, etc., and it was then found that some of these were sufficiently accurate for "reference" gauges. In this connexion, it might be pointed out that the speaker's firm was constantly introducing lathes of the best make for the tool-room, and a certain number of the accurate lathes were devoted solely to finishing screw-gauges.

With regard to the second point, accurate means were developed for producing cutting tools to the correct shape, and for setting these tools up truly in the lathe.

With regard to the third point, after much experimenting, the following sequence of operations was found to be satisfactory in the manufacture of case-hardened mild-steel gauges: (*a*) rough-turn a piece of steel; (*b*) anneal; (*c*) rough-cut gauge—finish threads to within, say, 0·010 inch; (*d*) carbonize to depth of about 0·030 inch; (*e*) finish-cut the gauge; (*f*) harden; and (*g*) lap. With these operations, it was found that very little lapping was necessary, and that practically no distortion took place in hardening. Of course, it went without saying that the hardening furnaces should all have pyrometers fitted, which should be periodically checked. In these hardening furnaces coal-gas was used, and care was taken that the gauge was properly suspended in the furnace.

With regard to the fourth point—suitable measuring appliances—a Pratt and Whitney measuring machine was used at the speaker's works, and attachments had been added to it similar to those in use at the National Physical Laboratory. Since the adoption of the system mentioned above, and since the men have got accustomed to the work, they had been able to get through as "reference" gauges about 75 per cent. of those produced. The majority of these screw-gauges were metric pitches which had been

made on lathes with so many threads per inch, the reduction being made by 127-toothed wheel.

He emphasized the importance of the amount of care that must be taken in the manufacture of these gauges, and stated that, in his opinion, to produce satisfactory results a competent man would have to devote his whole time to it. He pointed out that methods for the manufacture of these screw-gauges were still in their infancy, and interesting developments might be expected before long.

The Chairman (whose remarks were illustrated by blackboard demonstrations) said that, as a lathe manufacturer for many years, he might perhaps be permitted to express his high appreciation of the results in gauge manufacture which the London County Council Institute had accomplished, considering the tools with which the Institute was equipped, as described by Mr. Cooke. For work of such extreme exactitude as that described, special machine-tools were needed, and he spoke after many years' experience as a practical machinist. For work such as the Paper dealt with, the ordinary commercial lathe was not at all suitable, and was not intended for such a purpose, and it was not to be expected that extreme accuracy could be obtained at the price some people expected to purchase lathes. For accurate work good tools were needed, and unwise economy could only result in spoiled work and often much ultimate sacrifice of profit. For screw-gauge manufacture an entirely different type of lathe was required, roughly as the sketch, having an arrangement of slide with ample bearing surface instead of the ordinary saddle, and a simple rest with cross adjustments for putting on cut, and a short stiff guide screw-driven from the front end of the spindle of the lathe, in order to eliminate the errors due to tortional strain on the ordinary lathe where the screw was driven from the rear end of the spindle with a more or less unsatisfactory swing-frame arrangement connecting up to the screw. Gauges usually were quite short, not many inches long, and such a lathe would be admirable for the purpose.

The question of the accuracy of screw-threads was so important

(Mr. Walter Deakin.)

to the country's successful engineering operations that it was quite impossible to give too much attention to it. The British machine-tool trade in this country was, he regretted to say, behind that of some other countries in the character of its products. This was shown by the large purchases which had had to be made of high-class lathes and other tools for munitions manufacture from the United States, and he did not know that he should be entirely wrong if he was tempted to go so far as to describe the situation, at any rate, of some branches of the British machine-tool trade as "paltry." When visiting America and going through some of the machine-tool shops there, he was struck by the extraordinary care which they exercised in the matter of screw-thread manufacture, and this contributed considerably to the general excellence of their mechanical product. If the war had brought about no other advantage to the engineering trades, than to draw attention to the necessity for greater accuracy in our mechanical manufactures and impressed upon us the importance of scientific and practical improvement, the conflict would not have been without its benefits.

With regard to the question of lapping gauges, he had been very much interested in all that Mr. Cooke had had to say, and he had followed his remarks very closely, but it occurred to him after listening attentively that it was very probable Mr. Cooke and his co-experimenters had not yet arrived at the ultimate results. The Paper was, however, convincing upon one point, namely, that the Institute had accomplished some very real practical work, and had turned out instruments which had no doubt been of immense service to many engineering firms in the urgent business of munitions productions. Mr. Cooke had told them that one of the main objects of his Paper was to establish the claim of the Institute that they were more than a mere educational body—that by their instruction they could render a valuable aid in the everyday practice of engineering as observable in the country to-day from one end to the other, and he (the Chairman) believed that the members would agree with him that Mr. Cooke had made out a good case. Mr. Cooke at the outset admitted that in some practical engineering circles—and these not always unimportant—the

endeavours of the London County Council in this matter were looked at somewhat askance. He would not attempt to deny that the Author's suspicions were groundless. It was not, however, always the fault of those engaged in technical instruction that they were unable immediately to fulfil all the expectations formed of them. For one thing, satisfactory work could not be done without adequate tools, and there were cases in which technical instruction workshops suffered from want of sufficient up-to-date plant. They could see by the Paper that the Paddington Institute had been handicapped at the first by this insufficiency of equipment. It was there stated that "practically for all but the roughing-out operations complete reconstruction of those parts of the lathe on which precision depends," constituted the initial business to which Mr. Cooke and his fellow instructors had to turn their minds. Instead of standing aloof and criticising Technical Institutes as some engineers in the kingdom were inclined to do, it would he thought be very much more to the purpose if these people evidenced sufficient interest in the objects which the Technical Schools had in view and assisted towards their attainment. It should be a matter of honour to the engineering trades to see to it that that those in charge of centres of mechanical scientific instruction like the Paddington unit—as an example—were equipped adequately with the plant which was required, and to resolve that, in an Imperial crisis like the present, any help that these Institutes could render should be cordially accepted.

Mr. P. J. WORSLEY, Jun., said that members and visitors alike who had attended the meeting had gone there expecting to learn something valuable, and they had certainly not been disappointed. The Council in London were to be heartily thanked for having provided the opportunity of the Meeting, and it had been a real treat to listen to the able handling by Mr. Cooke of quite a new subject. He would like to possess rather more particulars with regard to the Institute's procedure respecting the testing of internal gauges. At his works they had been testing with a set of three plug-gauges: one cut away at the sides to fit the full diameter only;

(Mr. P. J. Worsley, Jun.)
one cleared top and bottom of the thread, testing the effective diameter only; and a cylindrical gauge from the bottom of the thread. By testing each of these three elements separately, considerable improvement in accuracy had been obtained.

Mr. EDWIN BUSWELL mentioned, with regard to limit gauges, that it was his custom to test before and after hardening the steel. He found that this method gave him some idea how the material would go up and down, and he took care that certain gauges were cut from the same bar of steel. It was his custom, when making screw-gauges from case-hardened mild-steel, to take a bar of steel and cut off one more piece than was required. This piece was used as a guide, then turned and the thread rough-cut, leaving about one ten-thousandth above size. The pieces were then sent to be carbonized, and one piece would be finished on the thread, and a test for size carefully taken before hardening; after the hardening process it would again be tested: the difference between the two tests would give an idea whether the material would increase or decrease. This method he had found very satisfactory.

Mr. W. TURTON, replying to an inquiry as to the effect of tempering case-hardened and mild-steel, said he anticipated that it would be found that the tempering of mild-steel gauges was no advantage, but rather to the contrary. The risk was that in the tempering process the gauges became distorted, particularly if there had been any overheating in the original hardening. His experience was that mild-steel properly carbonized did not require any subsequent tempering.

Lieut. W. R. PARSONAGE, R.N.V.R., remarked that they had all been greatly entertained by the Authors' description of the process of optical projection as used at the Paddington Institute. He noticed that Mr. Cooke stated that enlargement by this method was commenced for adjustment of the shape of tools for screw-cutting, and for ascertaining that the shape of the thread was correct, and that it was owing to the work by the Institute that

the process might be put to more extended use (so great was the accuracy of measurement that it provided), that other gauges such as plate-gauges of shape, slots and holes, curvature and cone angles, etc., were now adjusted by the same method. These particulars were very interesting, and opened up a field of thought which he was quite sure the Authors had not exhausted. From the marked success which the Authors had attained, he was certain that further investigation along these lines would be well repaid. He made no question that the application of this process to other intricate engineering measurement work would be extremely serviceable. He hoped that Mr. Cooke would give the Meeting the pleasure of listening to a second Paper from him, confining it to the subject of optical projection applied to engineering measurements.

An enormous amount of steel-hardening was being carried on in Birmingham at the present time, not only in actual trade practice, but also in experimental research. The Gas Committee of the City Council had established an Industrial Research Department and workshops where observations on steel-hardening were now being made. Various classes of steel supplied direct by the makers were regularly subjected to heat treatment for the purposes of observation and instruction, and careful records were kept of all results. These results had been tabulated, and were at the disposal of anyone who cared to consult them for assistance in engineering or other work. One point which the experiments had established was that care should be exercised that the material was heated clear of the muffle walls. Contact with the walls of the muffle meant lack of uniformity in heating up and distortion of the material. If the material did ultimately come to a uniform temperature, the distortion due to early uneven heating still persisted. A few days ago he witnessed some work of this kind being carried on, the steel bar being heated up to 850° C., and the material came out actually bright with a total absence of scale on the surface. The muffle was a vertical one with a coal-gas seal, and the material was held suspended in the centre of the muffle by a wire. The muffle could be either electrically or gas heated. The material was heated

(Lieut. W. R. Parsonage, R.N.V.R.)
and cooled without coming into contact with the atmospheric air while hot, and was in an atmosphere of coal-gas. An extension of the lower end of the muffle dipped into a bosh in which the material was cooled.

Mr. COOKE stated that he believed that scaling was often the cause of distortion in the material heated.

Lieut. PARSONAGE said the Birmingham Industrial Research Department method had another advantage, in that it did not decarbonize the surfaces in heating, but rather carbonized them. By the introduction of a small quantity of ammonia gas with coal-gas, a much greater carbonizing effect was obtainable, and mild-steel round gauges, screw-gauges, or flat gauges could be carbonized in this muffle.

Mr. A. G. COOKE, replying to Mr. H. Thompson, exhibited samples of cast-iron laps for plug- and ring-gauges. He pointed out that they covered at least half-a-dozen threads, and were adjusted with a screw to grip the gauge with very little pressure. The lap or gauge was held quite loosely by the hand to prevent rotation, but not to exert axial or transverse pressure. The shaping of the lap alone determined whether crest, core, or flanks were rubbed. The precision of the distance of rotation—about $2\frac{1}{4}$ revolutions was actually used—prevented any possible accident, and none had occurred after prolonged use with speeds up to 500 revolutions per minute.

In reply to Mr. Worsley, he stated that the lapping process was used after hardening to finally adjust the diameter and shape of threads, as well as to correct any residual periodic error in pitch. It would have no effect on progressive error. There had been no difficulty due to heating; the lapping of a plain plug would not differ in this respect.

In reply to Mr. Heathcote, he stated that he had attempted to use optical projection for pitch measurement, particularly for periodic variation; but there was a difficulty owing to the line

of measurement being parallel to the axis, and not perpendicular to the threads. The need of certainty and high accuracy in this fundamental measurement for gauge work necessitated an instrument such as was described in the Paper. Graphic records of the pitch were taken in gauges before and after hardening, and again after lapping, and formed a valuable guide. He preferred the cartridge paper to the semi-transparent screen as used by Mr. Heathcote, not only as a better material for accurate drawing of outlines, but because it was visible while operating the adjustments of the arc-lamp and lenses. The bromide paper print of screw silhouette exhibited showed that critical definition could be obtained with 166 magnifications. In fact, as the problem was not that of focussing as in ordinary projection or photography, the sharpness of the gauge was not reduced—at least, to the same extent —by the increase in magnification. The real difficulty was in producing a truly parallel beam of light and reducing diffraction; the latter required the screen to be at the shortest possible distance.

Replying to Mr. Armitage, he said he had tested a number of lathes intended for ordinary machine-shop practice, and found in all cases a progressive shortness of pitch of between 0·001 and 0·002 inch per inch. He much preferred direct correction by change of gear-wheels to any method of compensation, and found this possible to the degree of accuracy required. It was also practicable by this means to give a definite reduction in pitch which would allow for subsequent expansion in hardening. By systematic pyrometer measurements, the latter could be predetermined with certainty, though amounting in some cases to 0·001 inch.

In answer to Mr. Johnson, the alum bath appeared to absorb all heat rays, without apparent reduction of light, with a breadth of about 3 inches; probably less would be sufficient. Without this bath there would be an expansion of the gauge under heat, though perhaps the shape of a screw-thread would not alter. An ordinary grip-chuck was used, and a motor that would normally run at 1,200 revs. reduced by reversals to less than half. He used a hand-feed arc-lamp with carbons at right angles. There was a

(Mr. A. G. Cooke.)
much better arc-lamp introduced a few years ago, with a magnetic field to direct the arc and means of rotating the positive carbon to give symmetrical burning. This was a German import, and he had not been able to obtain one.

Mr. Pedley had raised the question of ring-gauges. For these the practice had been to make a correct plug check-gauge, and use a correctly shaped cutting tool and cast-iron lap. Whether the result was satisfactory was a question for the examiner rather than the gauge-maker, and he could not give an expert opinion. If any doubt existed, the shape could be verified most satisfactorily by a mould in Babbit metal.

He was glad that the discussion had largely turned on the question of the hardening of screw-gauges, although little had been said in the Paper. Having developed a lapping process for adjustment after hardening, the expansion and distortion that seemed inevitable in the use of cast or double-shear steel did not present the same difficulties that occurred to the manufacturer who relied entirely on the accuracy of the lathe. By the lapping process the diameters and shape of thread could be adjusted and small periodic errors in pitch eliminated, and the process was most rapid and certain. Only the introduction of progressive error in pitch by expansion in hardening had to be predetermined and allowed for, and this would have been much easier if a uniform quality of steel had been provided. He was nevertheless very glad of the information that had been given of the use of case-hardened mild steel, which in some cases might have distinct advantages, though lapping would still remain a valuable process for both accuracy and rapidity of working.

The CHAIRMAN moved that the best thanks of the Meeting be accorded to the Authors for their particularly interesting Paper. Their thanks were also due to the Council of the Institution for granting that Meeting, and giving them an opportunity of learning at first-hand what was being done at the Paddington Institute in so important a business as was now engaging the utmost energies of the engineering trades of the country. He was gratified that

the Paper had been provocative of so instructive a discussion, and he hoped that there would be an equally free exchange of views among the members at any future Birmingham Meetings which might be held. The Birmingham members must remember that it lay in their own power to decide how frequently discussions should take place in their midst. The Council of the Institution had made it abundantly plain that they were anxious to decentralize the debates of the Institution, wherever a sufficient desire existed among members in the country for the holding of provincial gatherings.

Mr. John W. Hall, in seconding the vote of thanks, said that he felt sure that the members had greatly enjoyed the Paper, and would gladly avail themselves of the opportunity to express their appreciation. He understood that, had the apparatus employed for projection work not been in constant use at the workshops in London, it would have been exhibited there that evening, and he would be very pleased if it could be exhibited at some future Meeting.

Mr. Cooke, acknowledging the vote of thanks on behalf of his coadjutors and himself, stated that the London County Council had been engaged for something like twenty-five years in efforts to impart technical instruction in engineering and closely kindred matters at their schools and workshops, and they were anxious above all things to obtain the confidence of the engineering industries of the country, and that these trades might realize that the Council was doing really practical and useful work. He thought that what he had been able to tell them in Birmingham should convince even the most sceptical that the claims of the Council were not baseless. The crisis through which the country was now passing had afforded the London County Council an opportunity of proving the worth of their classes such as had never before been possible, and he sincerely believed as the months went on this fact would come to be more and more realized and admitted by engineering firms.

(Mr. A. G. Cooke.)

The Chairman had alluded to the high degree of perfection which had been attained in the production of machine-tools in the United States. Upon this, he would like to say that it had been stated, with a considerable show of authority, that this skill had certainly followed upon the war between the Northern and Southern States of the Republic. The making of munitions for that campaign had led to the initiation of a very perfect system of manufacturing interchangeable parts, and he hoped that some similar method of standardization might follow from the experience which British engineers were now gaining in munitions output.

Communications.

Dr. R. T. GLAZEBROOK, C.B. (Director of the National Physical Laboratory), wrote that the Authors had produced an interesting and valuable contribution to the methods of screw measurement; they might, however, with advantage, have made it clear that much of the information contained in the Paper was obtained by them from the National Physical Laboratory, and that much of their success was due to the assistance received from the staff of the Laboratory. Thus (page 49), "the Authors found that the lathes by a number of manufacturers showed an error in pitch . . ." Some twelve test-screws and many sample gauges were measured at the Laboratory for the Authors, and this statement was based in great measure on the result of these measurements. Doubtless the Authors had had further evidence of their statement, but the work of the Laboratory might have been mentioned.

Again, Figs. 2 and 3 (pages 52–3) gave the results of pitch measurements which the reader would infer, he thought, were made by the machine shown in Fig. 1 (page 51). This, however, was not

the case. Measurements, with such a machine, could not be made to the required accuracy. Fig. 2 was plotted from results on a screw tested for pitch at the Laboratory on 26th Nov. 1915, while Fig. 3 was a diagram from a Laboratory Report of 4th April 1916. The diameter measuring machine, Fig. 4 (page 55), was a close copy of one designed at the Laboratory, described in the Laboratory Pamphlet of Dec. 1915, and since made in quantity by Messrs. Taylor, Taylor and Hobson, Ltd., while the projection apparatus in use at the Laboratory was described to two of the Authors very fully some twelve months since.

The Laboratory existed for the public benefit, and its staff were always ready to assist investigations to the best of their ability—as a fact, the Paddington Technical Institute had had more assistance than any other individual or institution—but it was only right that when the results of such assistance were used, as in this instance, in a public manner, the fact should be definitely acknowledged.

Mr. W. H. Newman wrote that his experience had apparently been very similar to the Authors', in the difficulty found in getting the ordinary lathe into a sufficiently accurate condition for screw-gauge work. A bad periodic error was, after a considerable amount of trouble, traced to irregular spacing in the teeth of a change-wheel, which was perhaps rather an unusual source of error.

One of the points which the writer had found to be most troublesome in cutting screw-gauges was in connexion with the wear of the tool edges. If an ordinary Whitworth form tool were examined under a microscope after a few fine cuts had been taken over a screw, it would be seen that the edge had been uniformly ground away, the result being shown much exaggerated in Fig. 27. The effect of this was to reduce the radius of the tool-point, and if two or three screws in succession had a few fine cuts taken over them with the same tool, it would be found that in each succeeding screw the effective diameter had risen relatively to the core diameter, showing that the radius of the tool-point was becoming smaller. This result seemed to take place very rapidly, and the

(Mr. W. H. Newman.)

tools constantly required correction to keep within the allowed tolerance. It would be of interest to have the Authors' experience on this point, more especially with regard to the steel used for tools, method of hardening, and clearance angles used.

The writer would hardly have thought that the arrangement shown in Fig. 5 (page 56) would be altogether satisfactory for measuring core-diameters, as it appeared difficult to ensure that the

FIG. 27.
Whitworth Form Tool.

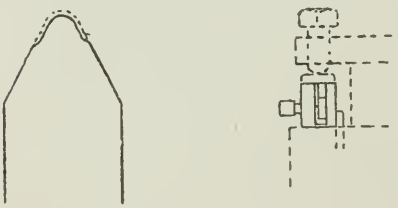
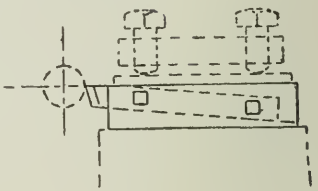


FIG. 28.
Tool-holder.



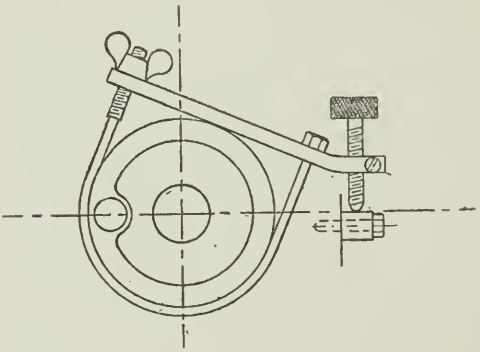


FIG. 29.
Arrangement for withdrawing tool when running the saddle back, and for feeding the tool in very small amounts.

maximum diameter was measured. He would have thought it necessary to have a third knife-edge on the upper side of the screw. The appliances shown in Figs. 28 and 29 had been found convenient, and might be of interest. Fig. 28 represented a simple form of tool-holder, which consisted of a rectangular block of steel, the depth being made equal to the height of the lathe-centres above the top of the slide-rest. A slot was milled down the centre of the block, the bottom of the slot making an angle with the

horizontal. The tool consisted of a flat bar of steel ground all over. In the writer's case it was $\frac{1}{2}$ inch by $\frac{3}{16}$ inch in section.

One advantage of this form of tool-holder was that the tool could always be kept at the height of centres by pushing it forwards in the block until its upper face touched a straight-edge placed on top of the block. Another advantage was that only a short piece of the tool on the top had to be ground or lapped instead of the whole length, as would be the case if the tool were horizontal. Two pins were provided in the top of the slide-rest, so that when the block was placed against them the tool was at right angles to the centre line. This enabled the tool to be quickly replaced after grinding.

Fig. 29 showed an arrangement which had been found very convenient for withdrawing the tool when running the saddle back and for feeding the tool in by very small amounts. The ordinary graduated ring provided on many lathes did not give a fine enough adjustment. A groove was turned in the edge of the hand-wheel on the end of the cross-feed screw. A stirrup surrounded this and passed through two holes in a cross-bar, and was tightened by a fly-nut. The bar had at one end an adjusting screw with a milled head, this screw abutting against a stop screwed to the saddle. In the writer's case, the proportions were such that one-fifth of a turn of the milled head corresponded to 0·0001-inch movement of the cross-feed, very approximately, which made a very convenient arrangement. By slacking the fly-nut on the stirrup, the hand-wheel could be moved as usual or the whole arrangement could be instantly removed.

The writer would like to mention the kind assistance he received from the National Physical Laboratory in measuring test-screws and in other ways.

Mr. A. G. COOKE wrote, in reply to Dr. Glazebrook, that it was stated in the opening of the Paper that "not only matters of organization and finance, but even reference to any other agency concerned, whether authoritative or auxiliary," would be avoided. This was in accordance with most definite instructions, which

(Mr. A. G. Cooke.)

could not on any account be disobeyed. The discussion having now indicated that the National Physical Laboratory was one of these authorities, to whom a large proportion of the gauges had been delivered, the assistance of the staff of the Laboratory in checking instruments and measurements in the process, as well as the final examination for acceptation, might be recorded. On the other hand, it might be pointed out that there had been no acknowledgment in either a public or a private manner by the various authorities for whom these services, including the manufacture of standards and instruments, had been freely and voluntarily given in this Technical Institute.

In reply to Mr. Newman, it was only after experiments with a number of different brands of steel that they found one satisfactory for screw-cutting tools. The tools were hardened right out in water, without tempering, and retained their cutting edge without appreciable change for a considerable number of gauges. The clearance angle given was 15 degrees. In measuring core-diameters, they had found no difficulty in insuring measurement of the full diameter by passing the screw-gauge through the micrometer.

The Institution of Mechanical Engineers.

PROCEEDINGS.

February 1917.

The Seventieth Annual General Meeting was held at The Institution of Civil Engineers, London, on Friday, 16th February 1917, at Six o'clock p.m. The Chair was taken by the Retiring President, Dr. W. Cawthorne Unwin, F.R.S., who was succeeded by Michael Longridge, Esq., the President elected at the Meeting.

The Minutes of the previous Meeting were read and confirmed.

The President announced that the following four Transferences had been made by the Council:—

Associate Members to Members.

Davies, Bertie Trevor, ...	Loughborough.
Dodds, William Mathison, ...	Penang.
Jackson, Sir Robert Montrésor, Bart., . .	London.
Willis, Edward, ...	London.

The Annual Report of the Council for the year 1916 was then presented.

The PRESIDENT, in moving the adoption of the Report and Statement of Accounts, said that pages 2 and 3 contained rather a long list of members who had died during the year. Amongst them it would be noticed with sadness, and yet with a tribute to the honour they had achieved, the names of some twenty-three who had died on active service. He thought it would be of interest to the members if he stated that the Secretary had compiled a statement from which it appeared that there were 955 members of the Institution known to be engaged on active war service. Of that number some 540 were engaged in services involving engineering knowledge and experience, namely, those in the units of the Royal Engineers, the Royal Flying Corps, the Mechanical Transport Corps, and as Inspectors of Machinery; 180 were in the Royal Field Artillery, the Royal Garrison Artillery and the Army Service Corps—again services in which engineering knowledge was useful; and 235 were engaged either in the combatant ranks or in services as to which no definite knowledge had been obtained. It was an interesting fact that so large a number of the members was actually engaged in the service of the country.

All the members had been much interested during the past week in the new War Loan. From a statement prepared by the Secretary, it appeared that the Institution had converted funds belonging to it into new War Loan to the extent of £16,620, and had taken up new War Stock to the amount of £8,750, so that altogether more than £25,000 (including the Benevolent Fund) had been invested in the new War Loan. With regard to the Accounts, he desired to mention, in case it escaped the notice of any of the members, that the total amount of the Institution's assets was about £137,000, and if the debentures, the temporary loan, etc., and the funds which had been put aside for the redemption of the Institution's leasehold property were deducted, there was still left about £75,000 as the net capital of the Institution. The Institution could, therefore, consider itself at the moment to be in a very solvent condition. He did not think there was anything further in the Report that he need advert to, and it was now open to discussion by the members.

Mr. Daniel Adamson suggested, for the consideration of the Finance Committee, that it would make the Accounts a little more easily understood if the amount which was set aside every year for the redemption of debentures and the Institution's leasehold property was dealt with in the Revenue Account rather than in the general Balance Sheet. The effect would be that the balance, which was called surplus of revenue over expenditure, would not be shown to be so large. That was not really a correct amount, because provision must be made every year for depreciation of the buildings, which were held on a lease. That provision was made in the Accounts, but his suggestion was that it should be shown in the Revenue Account in the future rather than in the Capital Account or in the Balance Sheet.

The President said that the Chairman of the Finance Committee was present at the Meeting, and would no doubt take into consideration the point that Mr. Adamson had raised. He now formally put the Motion that the Report be approved and adopted.

The Resolution was then put and carried unanimously.

The Secretary read the following list of Awards in connexion with the Papers read and Examinations held during the past year :—

Thomas Hawksley Gold Medal for the best Paper published in the Proceedings of 1916: To Daniel Adamson, *Member*. (Proceedings 1917, page 6.)

Thomas Hawksley Premium of £10: To Sec. Lieut. Robert W. Fenning, R.E. (T.), B.Sc., D.I.C. (Proceedings 1917, page 6.)

Starley Premium: To Robert E. Phillips, *Member*. (Proceedings 1917, page 6.)

Water Arbitration Prize: To Walter Clemence, *Member*. (Proceedings 1917, page 6.)

Institution Examinations: To L. H. Thomas. (Proceedings 1917, page 6.)

The President congratulated those who had won these prizes, and handed them their Awards.

The President reported that the Ballot Lists for the election of Officers for the present year had been opened by a Committee of the Council, and that the following were found to be duly elected :—

President.

Michael Longridge, ...	Altrincham.

Vice-Presidents.

Dugald Clerk, D.Sc., F.R.S., . . .	London.
Sir Robert A. Hadfield, D.Sc., D.Met., F.R.S.,	Sheffield.
Mark Robinson, ...	London.

Members of Council.

Sir A. Trevor Dawson, ...	London.
George Hughes, ...	Horwich.
Robert Matthews, ...	Manchester.
Donald B. Morison, ...	Hartlepool.
Sir Gerard A. Muntz, Bart., ...	Birmingham.
William Reavell, ...	Ipswich.

The Council for the present year is therefore as follows :—

President.

Michael Longridge, ...	Altrincham.

Past-Presidents.

John A. F. Aspinall, M.Eng., . . .	Manchester.
Sir Alexander B. W. Kennedy, LL.D., F.R.S., .	London.
William H. Maw, LL.D., ...	London.
E. Windsor Richards, ...	Caerleon.
W. Cawthorne Unwin, LL.D., F.R.S., . .	London.
Percy G. B. Westmacott, ...	Ascot.
J. Hartley Wicksteed, ...	Leeds.

Vice-Presidents.

William H. Allen,	Bedford.
Dugald Clerk, D.Sc., F.R.S.,	London.
Henry Davey,	Ewell.
Sir Robert A. Hadfield, D.Sc., D.Met., F.R.S.,	London.
J. Rossiter Hoyle,	Sheffield.
Mark H. Robinson,	London.

Members of Council.

Archibald Barr, LL.D., D.Sc.,	Glasgow.
Sir J. Wolfe Barry, K.C.B., LL.D., F.R.S.,	London.
Sir A. Trevor Dawson,	London.
John Dewrance,	London.
Sir J. Alfred Ewing, K.C.B., LL.D., F.R.S.,	Edinburgh.
Charles Hawksley,	London.
H. S. Hele-Shaw, LL.D., D.Sc., F.R.S.	London.
George Hughes,	Horwich.
Henry A. Ivatt,	Haywards Heath.
Bowman Malcolm,	Belfast.
Robert Matthews,	Manchester.
Donald B. Morison,	Hartlepool.
Sir Gerard A. Muntz, Bart.,	Birmingham.
Engineer Vice-Admiral Sir Henry J. Oram, K.C.B., F.R.S.,	London.
William H. Patchell,	London.
Walter Pitt,	Bath.
Sir Vincent L. Raven,	Woolwich.
William Reavell,	Ipswich.
Captain H. Riall Sankey, C.B., R.E., ret.,	London.
Wilson Worsdell,	Ascot.

(*One Vacancy to be filled up in March.*)

The President said he had now come to the end of his term of office as President, and it was his duty to ask Mr. Michael Longridge to take the Chair. Mr. Longridge had been a Member of the Institution for thirty-seven years, and had long been a member of the Council; he had been Chairman of one of the Institution's most important Committees, and he had earned the goodwill and the respect of all the Members with whom he had worked. Mr. Longridge had seen the rise and development of those Associations for the examination of steam machinery which were started by his (Dr. Unwin's) old chief, Sir William Fairbairn, in days when he was first learning something about engineering. Mr. Longridge had become the greatest master in the country on the pathology of steam machinery, and the work which those Societies had done had been of the greatest service in securing safety in the use of steam machinery. He (Dr. Unwin) could not say that he thought he was inviting Mr. Longridge to accept a sinecure; he thought Mr. Longridge would find that the Presidency would make considerable demands on his time and his judgment. Happily the long experience of the Institution had established rules and precedents which to a President eased the difficulties he was likely to meet. He wished the new President great happiness and great success during his occupancy of the Chair of the Institution.

The Chair was then vacated by Dr. Unwin, and taken, amid hearty applause, by the new President, Mr. Michael Longridge.

The President (Mr. Michael Longridge) said that on the present occasion it was not necessary for him to say much, as he would have to address them at length in a few weeks' time. He desired, however, to say that the kind words with which he had been inducted into the Chair had given him—as they would give any man—the keenest satisfaction. He was very grateful to Dr. Unwin for the kind way in which he had spoken of him, and to the members for the hearty manner in which they associated themselves with Dr. Unwin's remarks. He hoped that when the time came for him to retire, the members would give him as kindly

a farewell as they had given him a welcome that evening. He had only one regret at the present moment, namely, that the induction ceremony had not taken place in the Institution's own home. There the President was put, if not on a pedestal, at least on an elevated eminence, where the members could all look up to him as they ought to do. In the building of the Institution of Civil Engineers the positions were reversed: the President and his Council sat on the lowest seats. The welcome that had been given him, however, assured him that they were not looked down upon in any sense to which they could object. He hoped that before his term of office was over the Institution would have returned to its own home, and that the President and Council would occupy the seats of honour and eminence which had been provided for them there.

He was now about to call upon Dr. Maw to move a Resolution which he was sure all the members would wish to be passed. When he (the President) inducted Dr. Unwin into the Chair two years ago, he ventured to prophesy—and he ventured because he knew—how much the Institution would owe to its new President. He wished it could have fallen to himself to tell Dr. Unwin how fully this prophecy had been fulfilled; but custom ruled otherwise, and he therefore placed the duty in the hands of Dr. Maw, than whom no one could discharge it better.

Dr. William H. Maw (Past-President) said that, as the senior Past-President attending the Meeting, it was his great privilege to ask the members to pass a most hearty vote of thanks to the retiring President for his services to the Institution during the past two years. Dr. Unwin was pre-eminently a representative of the Mechanical Engineering Profession. There were very few, if any, of the members who had not at some time or other been more or less directly indebted to Dr. Unwin for information and for aid. Dr. Unwin's work in connexion with the development of technical education was well known; its results had been important and far-reaching, and the rising generation of engineers had all more or less benefited by the work he had done in making technical training

(Dr. William H. Maw.)

more practical. In the case of the older engineers the indebtedness was not less. Personally he could recall some hundreds of occasions on which he had had to refer to Dr. Unwin's writings for information of which he was badly in want, and he had never been disappointed in getting the information he required; it had always been stated clearly and accurately. He was sure all the members greatly regretted that Dr. Unwin's period of office had not fallen in happier times. As had been said by the new President, the members had been excluded from their own home, although that loss had been very greatly mitigated by the generous manner in which the Institution of Civil Engineers had received them at their home, and by the aid they had given in many ways. The Institution had also suffered from the depletion of its staff owing to the calls of the war; difficulties had been experienced in getting Papers as readily as would have been the case in ordinary times, and altogether a great amount of extra work had been thrown upon the President and upon the staff of the Institution. He need scarcely say that those calls had been most thoroughly met, but he thought only the Council could fully appreciate the great amount of work which had been done by the President during his term of office. The Institution had also been deprived of its social functions; no public Dinners, no Conversaziones, and no Summer Meetings had been held. The loss of the Summer Meetings was, he thought, particularly great, as they had been a striking feature of the work of the Institution since its early days, and they afforded admirable opportunities for intercourse between the members generally and the President and Council. It was, therefore, particularly to be regretted that Dr. Unwin had been deprived of the pleasure which he (Dr. Maw) was sure he would have derived had the usual Summer Meetings been held. He hoped, however, it would not be long before it was possible to resume them, and that at the future summer gatherings the members would have the pleasure of Dr. Unwin's attendance at the various excursions and social functions. He did not think it was necessary for him to say any more, because the members knew well how hard Dr. Unwin had worked in their service. He would,

therefore, conclude by formally proposing that a most hearty vote of thanks be accorded to Dr. Unwin for his services to the Institution during his period of office.

Mr. William H. Allen (Vice-President), in seconding the motion, said that Dr. Unwin had proved a model President. He had attended almost every Meeting of the Council during his Presidency, and he had thoroughly fulfilled the duties which it was so important that the President should carry out. The amount of detailed knowledge connected with the Institution that the President always had at his fingers' ends appeared to him to be quite remarkable. In addition to his work as President, Dr. Unwin had endeared himself to all the Members of the Council, and he hoped they would have the pleasure of seeing him very frequently at the Meetings as one of the Past-Presidents.

The Resolution was then put and carried with acclamation.

Dr. W. Cawthorne Unwin (Past-President) thanked the members from his heart for the very kind way in which they had received the Resolution, and Dr. Maw and Mr. Allen for the too generous words they had used in connexion with his work for the Institution. When he accepted the Presidency he said that they were entering on difficult times, and he thought they had proved in some respects even more difficult than could then have been foreseen. As Dr. Maw had said, the Institution had lost the use of its building. The Secretary had had to carry on the work of the Institution largely with a new and untrained staff, but he was sure the members would say that there had been no obvious defect in the work which had been effected in an office imperfectly staffed. Those functions of the Institution which were such a pleasant characteristic of it in bringing members together had had to be abandoned for the time being. He hoped that at any rate before long the times would be such that they could again be resumed. The Council came to know that amongst the effects of the war some of the members who were on active service were involved in various

(Dr. W. Cawthorne Unwin.)

difficulties, and new By-laws were therefore adopted which to some extent afforded the necessary relief. Candidates who had gone on active service were also handicapped in preparing for their examinations, and the Institution had modified its Examination Rules to provide for those particular cases.

He had come to a point at which he inevitably took a retrospect, and a retrospect was seldom without something of regret and of sadness. He felt there were things which might have been better done, and there were things which might have been done which had not been accomplished; nevertheless, he thought he might believe that no serious interest at the Institution had really suffered. The only other thing he desired to say was that he wished to acknowledge the extreme goodwill and help afforded him by the Council and by the Officers of the Institution. No President could carry on his work at the Institution without such goodwill and such help. He hoped he might take the hearty reception that had been given to the Resolution as some indication that he had the goodwill of the members of the Institution also.

Mr. E. R. DOLBY said he had much pleasure in proposing the following Resolution :—

> "That Mr. Raymond Crane, F.C.A., 46–47, London Wall, London, be reappointed to audit the accounts of the Institution for the present year, at a remuneration of Fifty Guineas."

Mr. DANIEL ADAMSON seconded the Motion, which was carried unanimously.

The following Paper was then read and discussed:— "Alternating Stress Experiments"; by WILLIAM MASON, D.Sc., of the University of Liverpool, *Associate Member.*

The Meeting terminated at Half-past Seven o'clock. The attendance was 69 Members and 26 Visitors.

Dr. Mason's Paper was read and further discussed at the Engineers' Club, Manchester, on Thursday, 1st March 1917.

ALTERNATING STRESS EXPERIMENTS.

By WILLIAM MASON, D.Sc.,* of The University of Liverpool,
Associate Member.

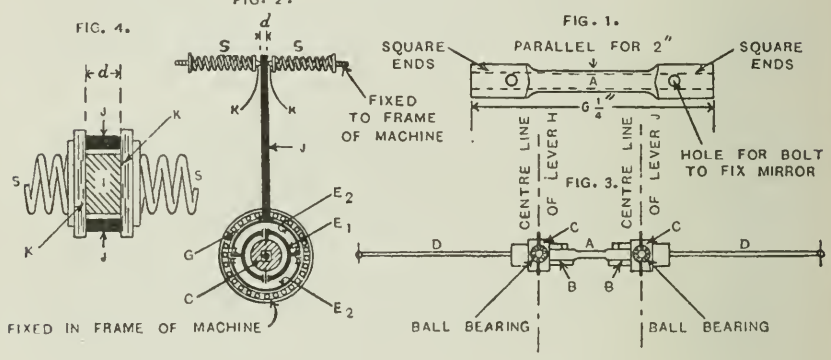
The main feature of these experiments is the systematic measurement of cyclic strains during complete alternating stress tests. The tests were of mild steel in alternating torsion and in alternating bending.

Machine used in Experiments.—The machine has been designed by the Author to make tests on specimens under repeated torsion, or bending, or under repeated torsion and bending simultaneously. It is a slow-speed machine; the work already done by its aid being at frequencies either less than, or very little exceeding, 200 cycles per minute. It appeared that measurement of cyclic strains and convenience in changing the frequency of the cycles without stopping and without alteration of stress, would be best obtained by adopting a slow speed. At such slow speeds as the above, vibration of the specimen and of the strain-measuring apparatus may be made negligible.

The specimen A, Fig. 1 (page 122), is secured between grips B, Fig. 3, which are keyed to the square ends of the specimen before it is put into the machine. These grips are bolted between the faces of a pair of sockets C fixed in the machine, Fig. 3 and Fig. 5, Plate 3. The method of gripping and fixing in the machine ensures that the specimen is quite free from initial indeterminate stress. Each socket C is capable of partial rotation upon a vertical pivot of $\frac{1}{2}$-inch diameter hardened steel. The ends of these pivots are secured in steel rings E_1, Fig. 2, while E_1 may rotate upon a

* Received Bryan Donkin Fund Awards in 1910 and 1913.

horizontal ball-bearing pivot which is fixed in an outer ring E_2. The horizontal and vertical pivots thus form a gimbal-joint supporting the socket C at its centre. The outer ring E_2 is surrounded by a ball-bearing G, the exterior of which is fixed in a casting bolted to the frame of the machine. A vertical aluminium lever is bolted to the ring E_2 of each gimbal-joint; the end of one of these levers H, Fig. 5, receives an oscillatory movement whose amplitude may be varied to any required degree of fineness while the machine is running; the end of the other aluminium lever J is held in position by the springs SS, Fig. 2 and Fig. 5, Plate 3.



Thus the lever H tends to twist the specimen against the measured resistance of the springs S.

The mode of action of these springs will be seen by reference to Fig. 4, which is a diagrammatic plan looking vertically down upon the top of the vertical lever J. J is in two halves, the halves being bolted to opposite vertical faces of E_2 and thus rigidly connected together. Between the tops of these two halves, but not touching either, is the block I, which is fixed by an adjusting screw to the frame of the machine. The dimension "*d*," Figs. 2 and 4, is the same exactly as the breadth of I and the breadth of the halves of J. Knife-edges KK are pressed by the springs SS upon J and I. When torque is put upon the test-piece A by the lever H, the top end of J tends to move against the pressure of one (or the other)

of the springs S, but movement of J cannot occur until the whole force of this spring is exerted upon J, and the block I entirely relieved of pressure from this spring.* The maximum torque upon the specimen is thus measured by the force of compression in S multiplied by the length of lever J.

A bending moment, uniform over the length of a specimen, may be applied by a pair of levers D, Fig. 3 and Fig. 5, Plate 3, of equal length, which are keyed into the cores C, Figs. 2 and 3, of the gimbal-joint. The horizontal ball-bearing pivots (already mentioned) between the rings E_1 and E_2 form the fulcra for the levers D. The outer ends of these levers receive a vertical oscillatory movement, and the amount of the bending movement given by them to the specimen is regulated by control springs R, Fig. 5, in a manner similar to that already described for the torque.

All the fittings described are in duplicate, so that two specimens may be tested at the same time. The approximate dimensions of the machine may be inferred from the lever J, Fig. 5, Plate 3, which is 16 inches long from the centre of lower end to the axis of the springs S.

Accuracy of Measurement of Torque and Bending Movement.—When the springs S, Fig. 2, have been screwed down by a calibrated amount required for a given torque, the amplitude of movement of the lever H is increased until the knife-edges K just lift away from the block I on either side at each to-and-fro maximum of the amplitude. In working, the amount of this lift on either side is shown by the breadth of the streaks of light from a lamp appearing between the knife-edges K and the block I. During tests, especially at very slow speeds, the lift, shown by the breadth of the streaks, is allowed, for the sake of convenience, to vary a little. As "fatigue" progresses, non-elastic cyclic strain increases, as shown by Bairstow.† This increase is shown by decrease of lift of the knife-edges K if the amplitude of motion of H remains the same.

* This spring device is similar to that used by Wöhler for measurements of stress in some of his machines.

† Phil. Trans. A. 210,

Since it is not always convenient to visit the machine at frequent intervals in order to regulate the amplitude of H, and so keep the lift of J exactly constant, the Author has been accustomed, upon occasions when the change of cyclic strain is slow, to increase the amplitude of H so that the lift of J is a little above the normal working amount, and to leave the machine at work without attention for a period. The lift of J then gradually decreases with the increase of cyclic strain. This variation of lift means a variation of stress so small as to be negligible in comparison with the total stress applied. The springs S have a stiffness of 11 lb. per inch of axial deflection. For example, with the springs screwed down to 12 lb. compression, and the torque therefore $12 \times 16 = 192$ inch-lb.—J being 16 inches long—a variation of lift of $\frac{1}{100}$ inch could be allowed (at very slow speeds) with a variation of torque of 1·75 inch-lb., or about one per cent. This was more than the extreme variation allowed in the experiments described, though, when change of cyclic strain was very slow, the Author has left the machine without attention for 36 hours. A lift of $\frac{1}{100}$ inch, as seen by the streak of light between K and I, looks large. It may be noted that a variation of 1 per cent. would correspond, in a rotating mass, or reciprocating mass testing-machine, to a variation of speed of half of 1 per cent. With a speed of 200 cycles per minute the variation of lift allowed was less, since the inertia of the lever J then becomes appreciable.

The inertia of the lever J about the axis of the specimen, and the effect thereof upon the measured torque, were calculated in designing the machine. Careful experiments have been made to ascertain the increase of torque, due to this inertia, imposed upon the specimen for a change of speed from 2 to 200 cycles per minute. In the case of specimen No. 27 (*see* Appendix, page 156) the calculated increase of torque for the above difference of speed is a little over 1 per cent. for the case when the torque is 140 lb.-inch, and the lift of the springs S 0·005 inch. This increase agreed with the increase of range of strain measured under the above conditions, the elasticity of the specimens being practically perfect. With the movement of J, that is, the lift of the knife-edges K, of $\frac{1}{200}$ inch,

allowed at the maximum speed of 200 per minute used in the tests of this Paper, it is improbable that the variation of torque with change of speed can have exceeded 1 per cent.

What has been said concerning the accuracy of measurement of torque applies generally, also, to that of the bending moment. There is a similar small increase of bending moment with increasing speed, which is small up to 200 cycles per minute, and is about 1 per cent. for the tests of this Paper. In the case of specimen B. 29, Table 4 (page 144), the increase of elastic deflection between speeds of 2 and 200 cycles per minute was measured. The lift of the springs was—as nearly as could be gauged—about twice the usual working amount, in order to obtain a larger difference for measurement; and the increase of bending moment corresponding to that of the elastic deflection was $1\frac{3}{4}$ per cent.

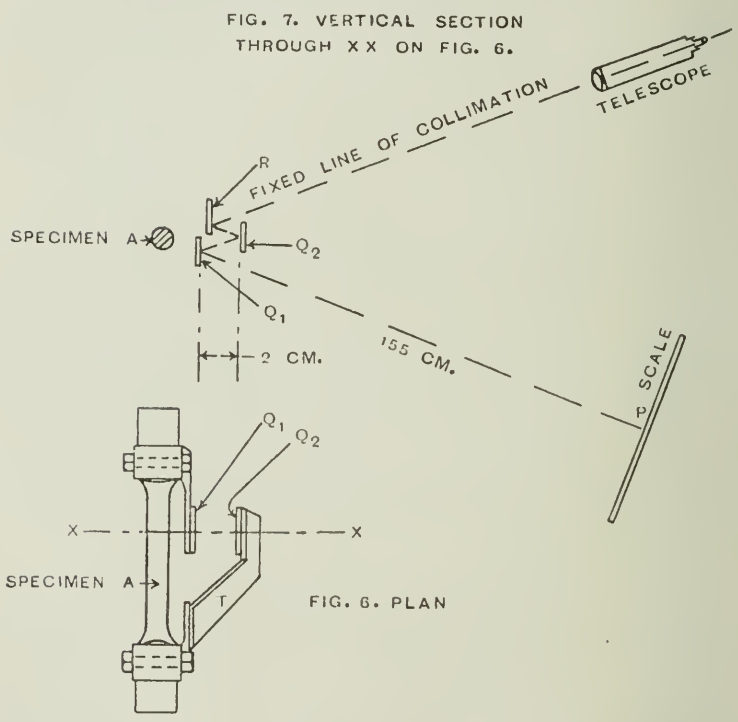
The only friction which can have any effect on the accuracy of measurement of the torque is that of the ball-bearing surrounding that gimbal-joint to which the lever J is fixed, Fig. 2 (page 122). When this ball-bearing was free of the grease with which it is covered in normal working, the friction is negligibly small. The grease is put on, not for the purpose of lubrication, but to keep damp from the ball-race. The moment of the "sticktion" of the grease is just measurable. No attempt has been made to make correction for it. The friction of the ball-bearing pivots, about which the bending levers D work, is quite inappreciable.

Apparatus for Measurement of Strains.—Torsional strains are measured by an arrangement of mirrors fixed to the specimen by bolts passing through the holes in the square ends shown in Fig. 1. The specimen is held horizontally in the machine. A mirror Q_1, whose plane is vertical, is secured to one end of the test-piece; a second vertical mirror Q_2 is carried by an arm T, Figs. 6 and 7, which is bolted to the other end. Q_1 and Q_2 face each other, and are about 2 cm. apart. Both are parallel to the axis of the test-piece. Fig. 7 is a vertical section through the line X X of Fig. 6 (shown underneath). R is a fixed mirror (not shown in Fig. 6), which is merely used for convenience of keeping the telescope and

scale on the same side of the machine. The scale is 155 cm. from Q_1, as shown in Fig. 7. As the specimen is twisted, the mirrors Q_1 and Q_2 alter their relative inclination; and the range of travel of the point P on the scale is proportional to the total angle of twist during a cycle of stress.

A similar arrangement, with two mirrors fixed to the specimen

Mirror Arrangement for Measuring Torsional Stress.



in vertical planes perpendicular to the axis of the specimen, enables the angular bending deflection between the ends of the specimen to be observed by means of another telescope and scale.

When a specimen is under alternating torsion, or alternating bending, the image of the scale travels vertically to and fro across the horizontal cross-wire of the telescope, and the difference between

the extreme readings of the cross-hair are thus proportional to the range of the strain. At speeds up to about 30 per minute, the extreme readings of the cross-hair may be read directly. At higher speeds, the motion of the image in the field of the telescope is too rapid for the scale divisions to be read directly; and the method of obtaining the upper and lower readings of the cross-wire is the following:—A black cover, fitted to slide along the scale, is provided with a narrow transverse slit, through which the white surface of the scale may be seen. The slit is illuminated by a lamp; and the black cover is slid along the scale until the image of that edge of the slit nearer to the middle of the travel coincides with the cross-wire of the telescope. At speeds approaching 200 per minute, the image of the slit is distinct only at the ends of its range of motion; the approximately simple-harmonic character of the motion, aided somewhat by persistence of vision, then enables the edge of the slit to be observed. The two positions of the slit on the scale, corresponding to coincidence of its edge with the cross-hair, give the readings whose difference is proportional to the range of the angular strain.

Material Tested.—The material of the tests has been provided by the Complex Stress Distribution Committee of Section G of the British Association. It is a "dead mild" steel of 0·12 per cent. carbon. The bars furnished to the Author were $\frac{15}{16}$ inch square. A direct tension test on a specimen turned to 0·623 inch diameter for a gauge length of 4 inches gave a yield-point 14·6 tons per square inch, a maximum stress $\left(=\frac{\text{maximum load}}{\text{area of bar}}\right)$ of 23·5 tons per square inch, and an elongation of 35·5 per cent. taken on 4 inches.* A torsion test on a solid bar turned to 0·372 inch diameter gave a yield-point of 9·90 tons per square inch shear stress, and the elastic limit appeared to be at about 5·8 tons per square inch.

* The elastic limit in tension appeared to be about 13·5 tons per square inch, but much reliance could not be put on this value because the specimen was held by ordinary testing-machine grips with no special apparatus for getting axial loading.

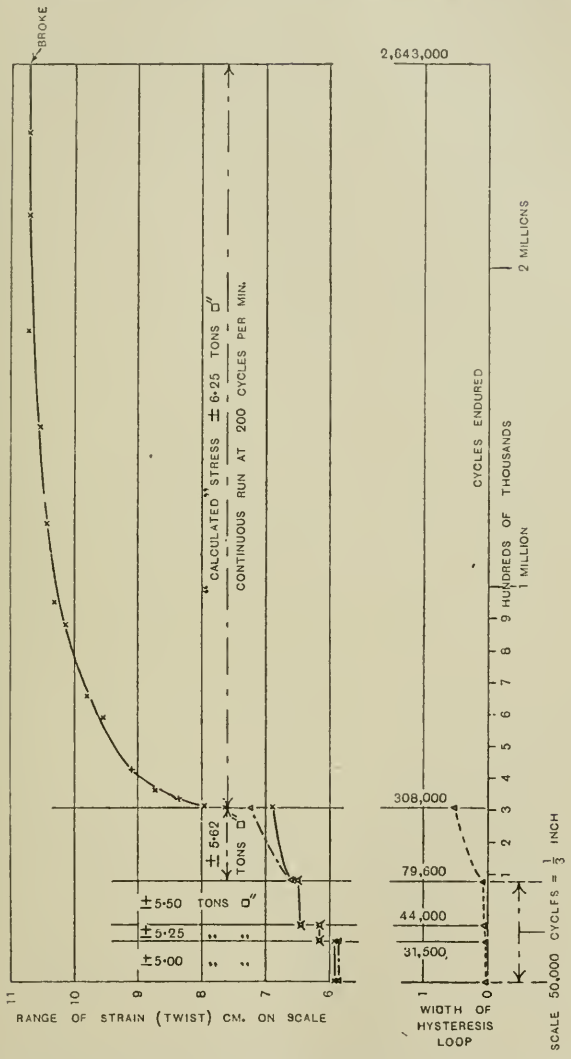
The specimens tested were cut from either of two bars, which are distinguished by the letters A and B. The numbers given to the specimens indicate the order in which they were cut from these bars. The material was not heat-treated in any way by the Author.

The Torsion Tests.—The behaviour of the material under repetitions of cycles of torsion of equal maximum torque in either direction of twist may be explained by reference to Figs. 8–12. Specimen B. 22, Fig. 8, was given a number of cycles at each of a series of low ranges of stress. Its elasticity was, so far as could be observed by the apparatus for strain measurement, sensibly perfect until the range of stress was $\pm 5{\cdot}50$ tons per square inch. At this stress, the width of hysteresis loop and the range of twist showed slight increases; * and at $\pm 5{\cdot}62$ tons per square inch (calculated from the torque by the formula which assumes perfect elasticity) it was evident that an elastic limit for cyclic applications of this stress had been passed. The range of torque was then increased to give $\pm 6{\cdot}25$ tons per square inch "calculated" (as above) stress; the range of twist now became greater very rapidly. After about $1\frac{3}{4}$ million cycles at this range of torque the range of twist had appeared to attain a maximum value, and after a total of about $2\frac{1}{4}$ millions at this range of torque the piece cracked. These cycles at $\pm 6{\cdot}25$ tons per square inch were imposed at the speed of 200 per minute and without stop or interruption of any kind.

The effect upon the range of torsional strain of rest and of change of speed of cycle is illustrated by the test of A. 10, Figs. 9 and 9*a* and Table 1 (page 135). In this test there was an elastic limit under repeated torque at or very near $\pm 5{\cdot}50$ tons per square inch;

* The width of the hysteresis loop affords a convenient measure of the cyclic plastic deformation. This loop is the curve obtained by plotting the strains against the corresponding stresses for a sufficient number of values of the latter throughout a cycle of stress. The width of this loop is thus, for alternating torsion, the difference between the readings of the angle of twist at successive half-periods of zero torque. The width is very nearly zero so long as elasticity is not impaired.

Fig. 8.—*Specimen B.22 (hollow). Alternating Torsion.* (See *Table* 2.)



Note.—Points marked thus Δ observed in dead-weight tests, the intermediate runs being at 200 cycles per minute.

Points marked thus × observed during runs at 200 cycles per minute.

Specimen A. 10, *Alternating Torsion* (*Tables* 1 *and* 2).

FIG. 9.—(See *Table* 2.)

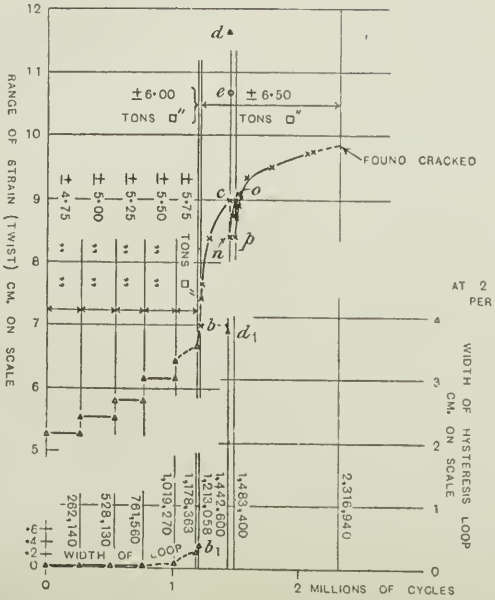
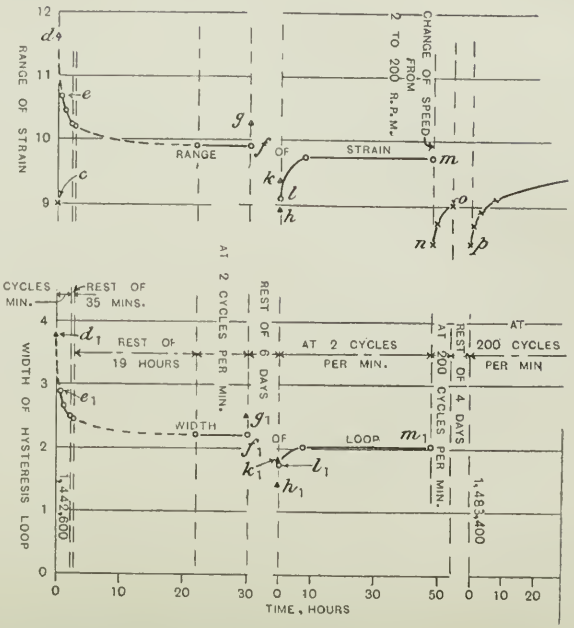


FIG. 9a.—*Curve illustrating Test between points "c" and "p" of Fig.* 9. (See *Table* 1.)

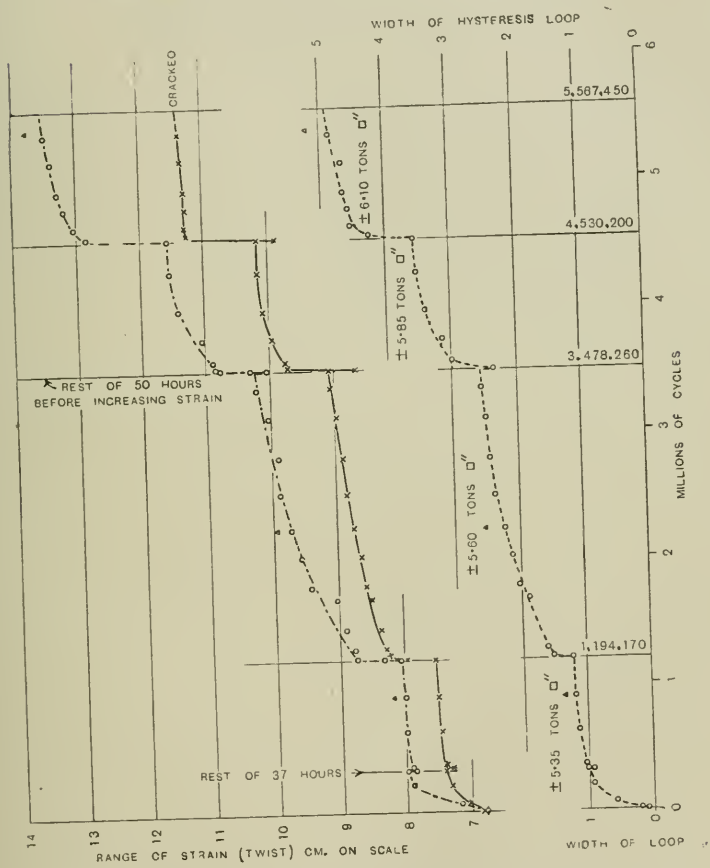


NOTE.—Points marked thus Δ obtained by dead-weight loading, the intermediate runs being at 200 cycles per minute.
Points marked thus × observed during runs of 200 cycles per minute.

Points marked o observed while machine was running at 2 cycles (= 2 r.p.m.) per minute.
Points marked Δ obtained by dead-weight loading.
Points marked × observed while machine was running at 200 cycles per minute (= 200 r.p.m.).

Fig. 10.—*Specimen No. B. 27 (hollow).* (See *Table* 2.)
Alternating Torsion Test at "Calculated" Stresses marked below.

Total time of Test, exclusive of rests, 19 days 3 hours.



Full Line Curve—Total Range of Twist between ends of Specimen. Speed, 200 per minute.

Dot-and-Dash Line—Total Range of Twist between ends of Specimen after change from 200 to 2 cycles per minute.

Dotted Line—Width of Hysteresis Loop (= difference of twist at successive half periods of zero torque) after change from 200 to 2 cycles per minute.

Points marked thus Δ determined by (slow) loading by dead-weights.

Fig. 11.—*Specimen B.* 28 (*hollow*). (See *Table* 2.)

Alternating Torsion Test.

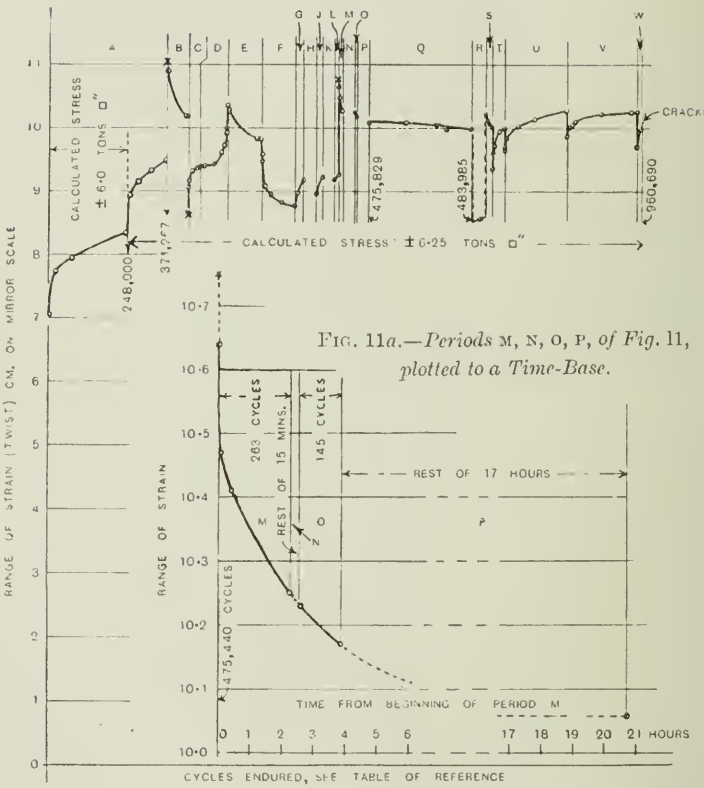


Fig. 11*a*.—*Periods* M, N, O, P, *of Fig.* 11, *plotted to a Time-Base.*

Note.—Points marked × were not observed; they indicate the probable ranges of Strain that would have been observed if the readings could have been taken instantly on change of speed.

after a run at $\pm 5{\cdot}75$ and a short run at $\pm 6{\cdot}00$, the range of $\pm 6{\cdot}5$ tons per square inch "calculated" stress was imposed, and the range of torque corresponding to that "calculated" range of stress was kept unchanged until the end of the test. Table 1 shows the series

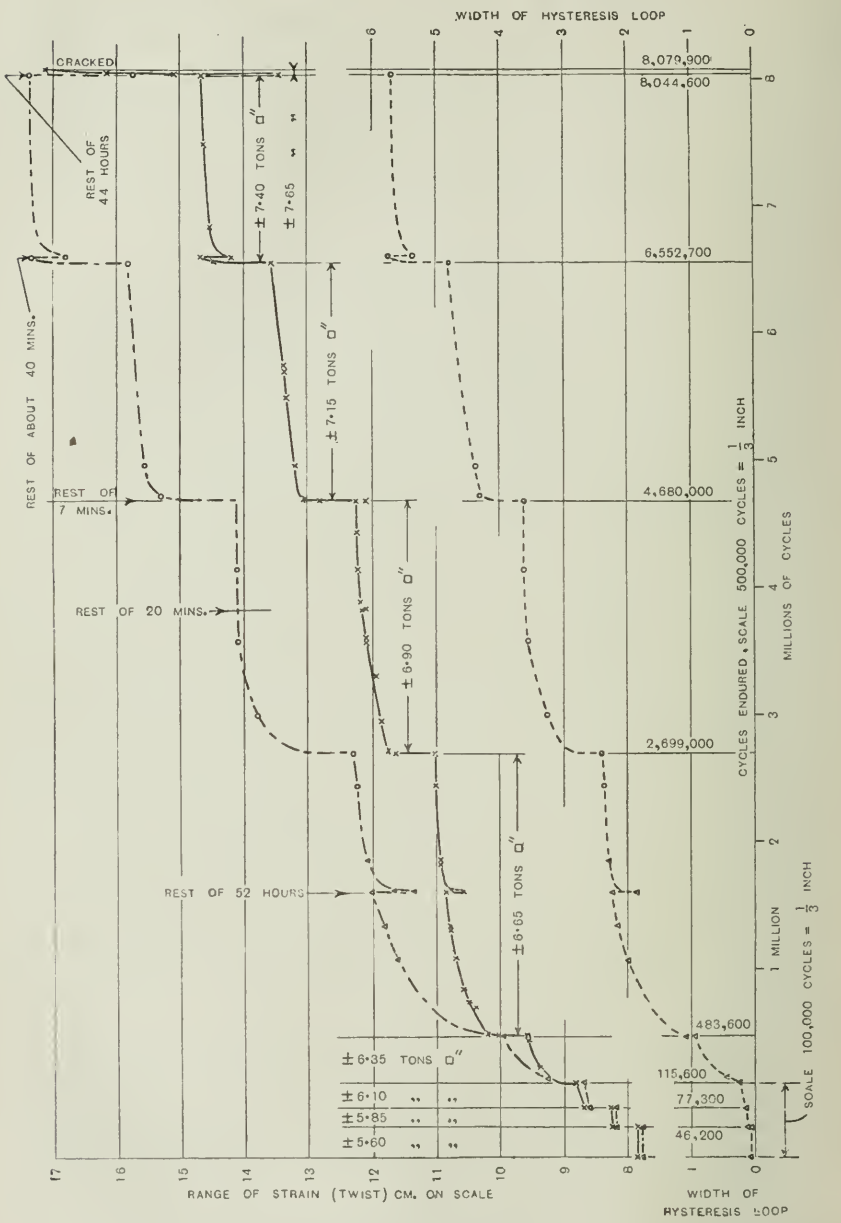
Table of Reference for letters on Fig. 11.

Letter on Figure	Period denoted by letter	Duration of Period	Speed during Period	Scale of Plotting of Cycles Endured
A	from 0 to 371267	31 hours	200 per min.	cycles 200,000 = $\frac{1}{3}$ inch
B	" 371267 " 372935	14·8 hours	2 " "	5,000 = "
C	" 372935 " 411580	3·2 hours	200 " "	200,000 = "
D	" 411580 " 413935	42 minutes	Speed decreasing from 200 to 2 p.m.	5,000 = "
E	" 413935 " 416600	23·4 hours	2 per min.	5,000 = "
F	" 416600 " 419300	38 minutes	Speed increasing from 2 to 200 p.m.	5,000 = "
G	" 419300 " 445268	2·2 hours	200 per min.	200,000 = "
H	445268	17 hours	Rest	——
J	from 445268 to 463296	1·5 hours	200 per min.	200,000 = "
K	463296	15 minutes	Rest	——
L	from 463296 to 475432	1 hour	200 per min.	200,000 = "
M	" 475432 " 475702	2·4 hours	2 " "	5,000 = "
N	475702	15 minutes	Rest	——
O	from 475702 to 475829	1·12 hour	2 per min.	5,000 = "
P	475829	17 hours	Rest	——
Q	from 475829 to 483985	72·2 hours	2 per min.	5,000 = "
R	" 483985 " 485090	5·5 minutes	200 " "	5,000 = "
S	" 485090 " 485585	4·4 hours	2 " "	5,000 = "
T	" 485585 " 523822	44·2 hours	14·4 " "	200,000 = "
U	" 523822 " 723450	118·5 hours	28·1 " "	200,000 = "
V	" 723450 " 945300	52·9 hours	70 " "	200,000 = "
W	" 945300 " 960690	1·25 hours	200 " "	200,000 = "

of operations done during the stage of the test between the points marked "c" and "p" of Figs. 9 and 9a. This test illustrates, also, the contraction in the range of strain and width of hysteresis loop during a run at a very slow speed subsequent to a change from 200 per minute to that slow speed.

(*Continued on page* 147.)

FIG. 12.—*Specimen No. A. 7 (solid).* (See *Table 3.*) *Alternating Torsion Test.*



Full Line Curve—Range of Strain (Twist between ends of Specimen) at Speed of 200 cycles per minute.

Dot-and-Dash Curve—Range of Strain after change from 200 to 2 cycles per minute.

Dotted Curve—Width of Hysteresis Loop after change from 200 to 2 cycles per minute.

NOTE.—Points marked thus Δ obtained by dead-weight loading.

TABLE 1.

Portion of Test of Specimen A. 10 *between cycles* 1,442,600 *and* 1,483,400. *"Calculated" stress* $\pm 6 \cdot 50$ *tons per square inch.*

(*See* Figs. 9 and 9*a*.)

Cycles from beginning of Test.	Reference Letter on Figs. 9 and 9*a*.	Procedure.	Centimetres on Scale.
1,442,600	*c*	Running at 200 r.p.m. Range of strain	9·00
1,445,816	*d*	Machine stopped. Range of torque applied by weights. Range of strain (hysteresis loop not closed) . .	11·65
		Machine started at 2 r.p.m.	
1,442,826	*e*	Range of strain at 2 r.p.m. .	10·69
		Running at 2 r.p.m.	
1,442,992	. .	Range of strain at 2 r.p.m. .	10·23
		Machine stopped. Rest of 35 minutes at zero torque.	
1,443,006	. .	Ran a few cycles at 2 r.p.m. to obtain range of strain at 2 r.p.m., which was . .	10·20
		Machine stopped. Rest of 19 hours at zero torque.	
		Machine started at 2 r.p.m.	
1,443,018	. .	Range of strain at 2 r.p.m. .	9·90
		Running at 2 r.p.m.	
1,443,698	*f*	Range of strain at 2 r.p.m. .	9·91
		Machine stopped.	
	g	Range of strain obtained with weights	10·28
		Rest of 6 days at zero torque, then 4 cycles, applied by weights :—	
1,443,698	*h*	1st cycle, range of strain, loop not closed	8·93
1,443,702	*k*	4th cycle, range of strain, loop nearly closed . . .	9·38
		Machine started at 2 r.p.m.	
1,443,712	*l*	Range at 2 r.p.m. . . .	9·10
		Running at 2 r.p.m.	
1,444,143	. .	Range at 2 r.p.m. . . .	9·73
		Running at 2 r.p.m.	
1,446,336	*m*	Range at 2 r.p.m. . . .	9·73
1,446,340	. .	Speed changed to 200 r.p.m.	
1,446,800	*n*	Range at 200 r.p.m. . . .	8·40
		Running at 200 r.p.m.	
1,483,000	o	Range at 200 r.p.m. . . .	9·02
1,483,255	. .	Machine stopped. Rest of 4 days under zero torque.	
		Machine started, and speed run up to 200 r.p.m.	
1,483,400	*p*	Range of strain at 200 r.p.m. .	8·40

TABLE 2.

Alternating Torsion Tests. Hollow Specimens.

No. of Specimen.	Preliminary dead-weight Test.			Range of "Calculated" Stress.*	No. of Cycles from beginning of Test.	No. of Cycles at Range of Stress.	Measurements obtained by dead-weight Loading.		Range of Strain at 200 Cycles per minute.	Range of Stress at which Strain ceased to be entirely elastic.
	Range of Torque.	Range of Stress.	Range of Strain.				Range of Strain.	Width of Loop.		
	lb. inch.	sq. inch.		tons per sq. inch.						tons per sq. inch.
A. 2	±223·0	±6·00	7·50	from ±5·9 to ±6·50	344,000	344,000				
					Rest of 14 months.					
				±6·10	0	251,000	—	—	to 7·98	
					251,000					
				±6·60	2,280,000	2,029,000	—	—	to 11·30	
				±6·85	2,541,000	307,400 — cracked.	—	—	to 12·95	
					2,587,400					
A. 10	±154·3	±4·75	5·27	±4·75	0	253,000	5·27	00·4		
See Fig. 9 (p. 130).					253,000					
				±5·00	528,000	275,000	5·54	0·04		
				±5·25	761,000	237,000	5·82	0·04	5·89	
				±5·50	1,019,000	258,000	to 6·16	from 0·04 to 0·08	from 6·23 to 6·28	±5·50

				±5·75	1,178,000	159,000	from 6·43 to 6·66	from 0·08 to 0·26	to 6·65	
				±6·00	1,213,000	35,000	to 7·17	from 0·26 to 0·33	to 7·00	
				±6·50	1,443,000	—	to 11·65	to 3·76	to 9·00	
					2,100,000	1,104,000				
					2,317,000	— cracked.	—	—	to 9·75	
A. 12	±152·0	(±5·4)	6·19	(±5·50)	0	18,500	from 6·19 to 6·73	from 0·07 to 0·33		
					18,500					
				(±6·50)	84,000	65,500	—	—	10·05	
					235,000	found cracked.				
B. 22	±154·0	±5·0	5·86	±5·00	000,000	31,600	5·86	0·03	5·90	±5·50
					31,600					
See Fig. 8 (page 129).				±5·25	43,900	12,300	6·15	0·04	6·15	
				±5·50	79,600	35,700	from 6·45 to 6·50	from 0·04 to 0·04	to 6·52	
				±5·62	308,000	229,400	from 6·59 to 7·24	from 0·08 to 0·54	to 6·90	
				±6·25	2,431,000	2,335,000	—	—	to 10·72	
					2,643,000	cracked.				
B. 25	±119·0	±5·0	5·90	±6·05	0	420,000	—	—	9·98	
					350,000					
					420,000	cracked.				
B. 26	±121·0	(±5·00)	5·22	(±6·25)	000,000	78,000	—	—	10·20	
					28,000					
					78,000	cracked.				

* These stresses are calculated from the range of torque on the assumption that the stress is proportional to the radius, in the section of the tube.

Frequency of cycles generally 200 per minute. Machine stopped for observations with dead-weight loading.

TABLE 2 (*continued*).

No. of Specimen.	Preliminary dead-weight Test.			Range of Calculated Stress.*	No. of Cycles from beginning of Test.	No. of Cycles at range of Stress.	Measurements obtained by dead-weight Loading.		Range of Strain at 200 Cycles per minute.	Range of Stress at which Strain ceased to be entirely elastic.
	Range of Torque.	Range of Stress.	Range of Strain.				Range of Strain.	Width of Loop.		
	lb. inch.	sq. inch.		tons per sq. inch.						tons per sq. inch.
B. 27 *See* Fig. 10 (page 131).	±131·0	±5·00	6·03	±5·35	0	1,194,000				
					1,194,000	—	—	—	to 7·48	
				±5·60	3,478,000	2,284,000	(*See* Fig. 12, page 134.)		to 9·06	
				±5·85	4,530,000	1,052,000	—	—	to 10·15	
				±6·10	5,567,000	1,037,000 cracked.	—	—	to 11·36	
B. 28 *See* Fig. 15, Plate 3.	±139·0	±5·00	5·93	±6·00	0	248,000				
					248,000	713,000	(*See* Fig. 13, page 148.)			
				±6·25	961,000	cracked.				
B. 30 *See* Fig. 15, Plate 3.	±169·5	±5·00	5·87	±6·25	000,000	940,000 cracked.	—	—	to 10·65	
					940,000					

* These stresses are calculated from the range of torque on the assumption that the stress is proportional to the radius, in the section of the tube.

Frequency of cycles generally 200 per minute. Machine stopped for observations with dead-weight loading.

TABLE 2 (*concluded*).

Diameters of Specimens.

No. of Specimen.	External.	Internal.	Thickness of Wall.			
			Max.	Min.	Mean.	Remarks.
	Inch.	Inch.	Inch.	Inch.	Inch.	
A. 2	0·500	0·377	0·0615	0·0615	0·0615	
A. 10	0·5705	0·507	0·0317	0·0317	0·0317	
A. 12	0·5725	0·510	0·038	0·025	0·0315	Bore very badly eccentric.
B. 22	0·5759	0·5142	0·0308	0·0308	0·0308	
B. 25	0·5595	0·510	0·0262	0·0232	0·0247	Bore eccentric.
B. 26	0·562	0·5095	0·0333	0·0195	0·0263	Bore very badly eccentric.
B. 27	0·5615	0·5072	0·0277	0 0273	0·0275	
B. 28	0·563	0·5053	0·0296	0·0280	0·0288	Bore slightly eccentric.
B. 30	0·5755	0·5053	0·0355	0·0345	0·0350	Bore slightly eccentric.

TABLE 3.

Alternating Torsion Tests. Solid Specimens.

No. of Specimen.	Preliminary dead-weight Test.			Range of "Calculated" Stress.*	No. of Cycles from beginning of Test.	No. of Cycles at Range of Stress.	Measurements obtained by dead-weight Loading.		Range of Strain at 200 Cycles per minute.	Range of Stress at which Strain ceased to be entirely elastic.
	Range of Torque.	Range of Stress.	Range of Strain.				Range of Strain.	Width of Hysteresis Loop.		
	lb. inch.	tons per sq. inch.		tons per sq. inch.						
A. 1 *See* Fig. 16, Plate 3.					0					
				±6·25	1,131,000	1,131,000				
				±6·50	2,304,000	1,173,000				
				±6·75	3,459,000	1,155,000				
				±7·00	4,009,000	550,000				
				±7·25	4,284,000	275,000				
				±7·50	4,507,000	223,000 broke.				
A. 5					0					
				±7·50	2,995	2,995 Test discontinued.	to 15·8	to 5·1		

A. 7 *See* Fig. 12 (page 134).	±182	±5·00	7·01	±5·60	0 46,200	46,200	7·71	0·06	7·84	between ±5·60 and ±5·85
				±5·85	77,300	31,100	from 8·05 to 8·19	to 0·11	to 8·26	
				±6·10	115,600	38,300	to 8·63	to 0·23	to 8·82	
				±6·35	483,600	368,000	to 9·82	to 0·95	to 9·56	
				±6·65	2,699,000	2,215,400	to 12·30	to 2·38	to 10·96	
				±6·90	4,680,000	1,981,000	to 14·02	to 3·56	to 12·24	
				±7·15	6,552,700	1,872,700	to 15·82	to 4·79	to 13·55	
				±7·40	8,044,600	1,491,900	to 17·29	to 5·60	to 14·67	
				±7·65	8,079,900	35,000 cracked.				
B. 31	±67	±5·00	9·53	±5·00	0 8,400	8,400	9·53	0·02		about ±5·60
				±5·25	10,700	2,300	10·16			
				±5·60	25,400	14,700	to 11·10	to 0·10		
				±5·95	56,600	31,200	to 12·06	to 0·55		
						Test dis-	continued.			

NOTE.—Frequency of cycles generally 200 per minute. Machine stopped while observations were made with dead-weight loading.

Diameter of Specimens:—	A. 1	A. 5	A. 7	B. 31
	0·435 in.	0·4335 in.	0·436 in.	0·3125 in.

* These stresses are calculated on the assumption that the stress over the section varies as the radius.

TABLE 4.—*Alternating Bending Tests. Solid and Hollow Specimens.*

No. of Specimen.	Preliminary dead-weight Test.			Range of "Calculated" Stress.*	No. of Cycles from beginning of Test.	No. of Cycles at Range of Stress.	Measurements obtained by dead-weight Loading.		Range of Strain at 200 Cycles per minute.	Range of Stress at which Strain ceased to be entirely elastic.
	Range of Bending Moment.	Range of Stress.	Range of Strain.				Range of Strain.	Width of Hysteresis Loop.		
	lb. inch.	tons per sq. inch.		tons per sq. inch.						tons per sq. inch.
A. 8				±13·00	0 164,000	164,000 cracked.	—	—	10·78 at 146,000 cycles.	
A. 14	±180·0	±10·0	5·36	±10·0	0 19,100	19,100	5·36	0·03		
				±11·0	19,100 34,300	15,200	6·00	to 0·10	5·97	Rather less than ±11·0
				±11·5	34,300 54,900	20,600	to 6·32	to 0·16	6·28	
				±12·0	54,900 78,400	23,500	to 6·88	to 0·36	to 6·72	
				±12·5	78,400 413,200	334,800	to 9·00	to 1·88	to 8·04	
				±12·75	413,200 908,500 963,000	— 549,800 broke.	9·87	2·46	9·00	
A. 16 *See* Fig. 14 (page 148).	±172·5	±10·0	5·13	±10·0	0 38,000	38,000	5·13	0·03	5·20	
				±11·0	38,000 75,000	37,000	from 5·63 to 5·75	from 0·06 to 0·16	from 5·67 to 5·72	Rather less than ±11·0
				±11·5	75,000 121,800	46,000	from 6·01 to 6·29	from 0·17 to 0·38	to 6·10	
				±12·00	121,800 193,000	71,200	from 6·63 to 6·97	from 0·44 to 0·68	to 6·66	

				±12·50	513,400	350,400	to 8·21	to 1·52	to 7·80	
				±12·75	813,700 to 959,640	— 416,240 cracked.	8·96	1·99	8·18	
A. 11 *See* Fig. 16, Plate 3.	±197·0	±10·5	6·60	±10·5	0 to 18,5000	18,500	6·60	0·03	6·70	
				±10·75		46,000	6·98*			
				±11·00	64,500 to 208,700	144,200	from *7·22 to 7·35		to 7 12	Rather less than ±11·0
				±11·30		46,000			to 7·42	
				±11·55	254,700 to 341,700	87,000			to 7·81	
				±11·70		60,900			to 7·92	
				±11·80	402,600 to 511,300	108,700			to 8·13	
				±12·05		108,700			to 8·40	
				±12·35	620,000 to 676,000	56,000			to 8·69	
				±12·65		109,800			to 9·05	
				±13·00	785,600 to 892,400	106,800			to 9·55	
				±13·35		103,200			to 10·10	
				±13·65	995,800 to 1,098,400	102,600			to 10·73	
				±14·30	1,194,300 to 1,400,000	301,600 cracked.	—	—	11·97	

(Ranges marked thus * were taken at 2 cycles per minute.)

* These stresses are calculated on the assumption that the distribution of stress is similar to that when the elasticity is perfect, that is, that the stress over the section varies as the radius.

Frequency of cycles generally 200 per minute. Machine stopped for observations with dead-weight loading.

TABLE 4 (*continued*).

No. of Specimen.	Preliminary dead-weight Test.			Range of "Calculated" Stress.*	No. of Cycles from beginning of Test.	No. of Cycles at Range of Stress.	Measurements obtained by dead-weight Loading.		Range of Strain at 200 Cycles per minute.	Range of Stress at which Strain ceased to be entirely elastic.
	Range of Bending Moment.	Range of Stress.	Range of Strain.				Range of Strain.	Width of Hysteresis Loop.		
	lb. inch.	tons per sq. inch.		tons per sq. inch.						tons per sq. inch.
A. 15 *See* Fig. 13 (page 148).	±177·0	±10·0	6·12	±11·00	0	48,000	from 6·74 to 6·89	from 0·03 to 0·11	to 6·96	Rather less than ±11·0
				±11·54	48,000	82,000	from 7·13 to 7·33	from 0·10 to 0·21	to 7·30	
				±12·00	130,000	76,000	from 7·64 to 7·84	from 0·21 to 0·37	to 7·72	
				±12·50	206,000	294,000	to 8·64	to 0·78	to 8·40	
				±13·00	500,000	307,000	to 9·61	to 1·27	to 9·20	
				±13·25	807,000	246,800	to 10·25	to 1·63	to 9·64	
				±13·50	1,053,800					
				±13·75	1,335,300	—	to 10·75	to 1·92	to 9·94	
					1,391,000	—	—	—	10·33	
					1,623,000	about 270,000 found broken.				
							Taken at 2 cycles per minute.			
B. 29	±106·0	±9·20	6·50	± 9·20	0	23,500	6·50			
				± 9·70	23,500	41,200	6·87			
					64,700					

±10·15	88,400	23,700	7 20			
±10·65	115,400	27,000	7·57			
±11·05	142,400	27,000	from 7·86 to 7·90	—	8·00	About ±11·0
±11·50	172,300	29,900	from 8·19 to 3·26	—	to 8·23	
±11·80	219,400	47,100	from 8·58 to 8·70	—	to 8·52	
±12·15	246,100	26,700	to 9·24	—	to 8·94	
±12·50	315,500	69,400	to 9·72	—	to 9·25	
±13·00	569,500	254,000	to 11·10	—	to 10·36	
±13·55	851,500	282,000	to 12·39	—	to 11·48	
±14·25	880,500	29,000		—	to 12·12	
±14·90	933,300	52,800	to 15·30	--	to 13·74	
		Rest of 17 hours.				
±14·90	973,400	40,100	to 16·17	—	to 13·97	
±15·60	986,500	13,100	to 17·88	—	to 15·18	

The piece was then given some intermittent runs, in all 50,000 cycles, with long intervening periods of rest. Piece became cracked at 1,020,000 cycles and broke at 1,036,400 cycles, the bending moment being that giving ±15·60 tons per sq. inch "calculated" stress.

* These stresses are calculated on the assumption that the distribution of stress is similar to that when the elasticity is perfect, that is, that the stress over the sections varies as the radius.

Frequency of cycles generally 200 per minute. Machine stopped for observations with dead-weight loading.

TABLE 4 (*concluded*).

Diameters of Specimens.

No. of Specimen.	External Diameter.	Internal Diameter.	Thickness of Wall.
	Inch.	Inch.	Inch.
A. 8	0·4375	0·3815	0·0280
A. 14	0·5003	0·3835	0·0584
A. 16	0·5765	0·506	0·0357
A. 11	0·436 solid	—	—
A. 15	0·432 solid	—	—
B. 29	0·3747 solid	—	—

Fig. 10 (page 131) of specimen B. 27 shows how the increase of range of strain, on change of speed from 200 to 2 per minute, varies with the extent of the range of strain. The width of loop at 2 per minute under the same conditions is also plotted.

Figs. 11 and 11*a* of specimen B. 28 illustrate a test made to find how changes between various speeds intermediate between 2 and 200 per minute affect the range of strain. The number of cycles endured in the various stages of the test is plotted to the different scales marked on Fig. 11, in order to show more clearly the effects of rest and of change of speed.

All the specimens previously mentioned were hollow, turned and bored, and of small thickness of wall. Similar results were obtained with solid test-pieces. Fig. 12 of A. 7 illustrates a test on similar lines to that of B. 27. The calculated stresses—that is, stresses calculated from the torque on the assumption that the stress varies uniformly with the radius—are higher than for the hollow specimens. The range of stress at which non-elastic strain appears is a little more difficult to determine for the solid than the hollow test-piece. For A. 7 this elastic limit appeared to be between $\pm 5{\cdot}60$ and $\pm 5{\cdot}85$ tons per square inch.

Tables 2 (pages 136–139) and 3 (pages 140–141) are lists of the torsion-tests done. All tests have been included (even those of hollow ones in which the bore was eccentric to the outer surface), except the first test done, namely, of A. 3, which is of no use for any comparison, since the contemplated procedure was upset by the then unsuspected large effect of change of speed.

Alternating Bending Tests.—Figs. 13 and 14 illustrate the tests in alternating bending done upon specimens A. 15 and A. 16, a hollow and solid test-piece respectively. Bending and torsion tests gave results similar in every respect. The bending tests are given in Table 4 (pages 142–146).

Mode of Fracture.—The fracture of the hollow specimens in alternating torsion were either always like that shown at A, Fig. 15, Plate 3, or if the crack was not far developed, like at B. It was

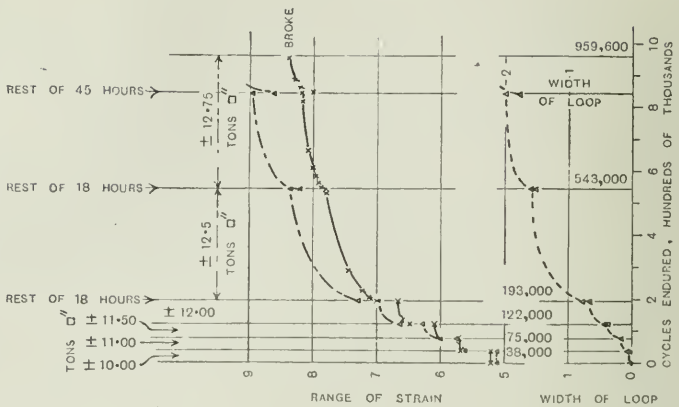


FIG. 14.—*Specimen A.16 (hollow). ± Bending.* (See *Table 4.*)

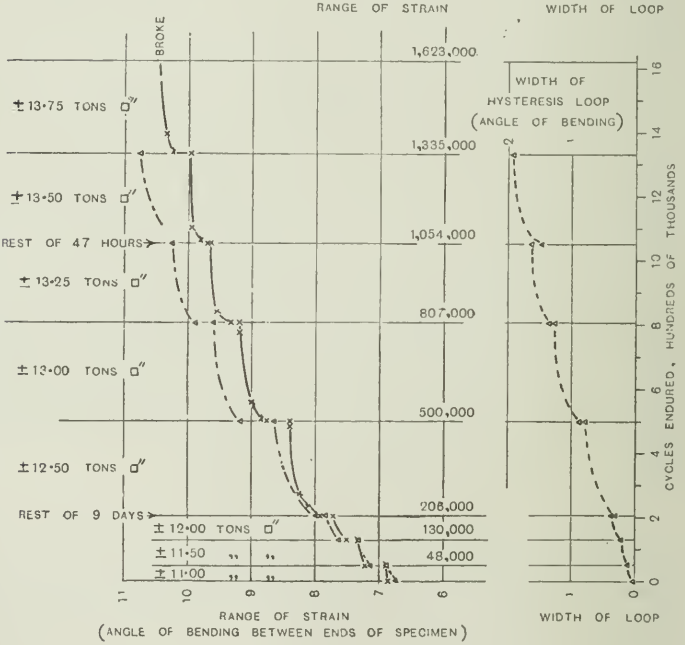


FIG. 13.—*Specimen A.15 (solid). ± Bending.* (See *Table 4.*)

Full Line Curves—Range of Strain at 200 cycles per minute.

Dot-and-Dash Curves—Range of Strain obtained by imposing bending moments by dead-weights, machine stopped.

Dotted Curves—Width of Hysteresis Loop (dead-weights, machine stopped).

clear that the straight part of the crack parallel to the axis was the first to appear, and that the pairs of cracks radiating from each end of this part were formed subsequently. The fractures of the solid torsion specimens were not all alike. That marked C, in Fig. 15, is exactly of the form of the hollow specimens, but on a smaller scale; that at D, in Fig. 16, has large serrations, and it is impossible to ascertain how the crack commenced. It was not as a rule possible to watch the development of the crack, because the strain-measuring apparatus obstructed the view; but as far as could be seen, it appeared that after the first hair crack was visible, complete fracture or a very large crack would be developed in 10,000 to 40,000 cycles.

The cracks in both solid and hollow specimens under alternating bending commenced along a short circumferential line. The crack became deeper and the piece would or did break in a plane perpendicular to its axis, as shown in Fig. 16 at E.

Discussion of Results.—Reference to Tables 2, 3, and 4 will show that even at comparatively small ranges of stress there was a small width of hysteresis loop. The order of magnitude of this width, however, indicates elastic hysteresis only.* The ranges of repeated stress at which elasticity began to be impaired were in the near proximity of $\pm 5{\cdot}50$ tons per square inch in specimens A. 10, B. 22, and B. 31; and in A. 7 about $\pm 5{\cdot}75$ tons per square inch of shear stress. In alternating bending, the ranges of stress were very closely $\pm 11{\cdot}0$ tons per square inch of direct stress; and thus the range of maximum shear stress was $\pm 5{\cdot}50$, the same, or very nearly the same, as for the alternating torsion. The agreement in these values suggests that shear stress was the prime cause of elastic failure in both alternating torsion and alternating bending, and that Guest's criterion of elastic failure holds for this steel.

A common feature exhibited by all curves of range of strain plotted against cycles endured, such as Figs. 8–14, is the rapid increase of the cyclic strain at the beginning of the application of

* *See* "Elastic Hysteresis of Steel." F. E. Rowett. Proc. Roy. Soc. A. Vol. 89.

cycles at a given range of torque or of bending moment (greater than that at which the elasticity remained unimpaired), and the subsequent much smaller rate of increase. In the case of solid specimens, or of tubular specimens with thick walls, such a shape of curve would be expected. For the real maximum stress induced by a given torque or bending moment will diminish as the cyclical non-elastic strain increases; just as, in static experiments, the real maximum stress in beams or shafts under a given loading tends to diminish when the elastic limits are passed in the regions of great stress. The "calculated" stresses marked on the graphs and given in the Tables are the real stresses only when the steel is perfectly elastic; and must be in excess of the real maximum stresses by an increasing amount as the cyclical non-elastic strain develops.

With thin tubular test-pieces, this excess may still be fairly large. Thus in B. 27, the maximum stress calculated on the assumption of perfect elasticity is $85 \cdot 3$ T where T is the torque. Calculated on the assumption that the shear stress is uniform over the area of the tube, the stress is $80 \cdot 3$ T—a difference of 6 per cent. upon the former; thus the real maximum stress of the last million cycles of B. 27 *may* have been only $\pm 5 \cdot 75$ tons per square inch instead of $\pm 6 \cdot 10$ as marked "calculated"-stress upon the graph of Fig. 10 (page 131). Moreover, since time is required for equalization of stress over the area of even one of the thin hollow specimens, it seems possible that the real maximum stresses at 200 per minute may be somewhat greater, on this account, than the real maximum stresses at 2 per minute. If this is so, the observed ranges of strain at 200 will be so much the greater, and the speed effect found herein so much the less than they would be for specimens in direct tension and compression.

The rapid rate of increase of cyclical non-elastic strain when a test is commenced, or when a given stress is increased, is exhibited by Dr. L. Bairstow's curves* of "cyclical permanent set" plotted against number of cycles endured for direct stress tests; thus it may be fairly assumed that the similar rapid rise and subsequent

* Phil. Trans. A 210. Page 38.

smaller rate of increase of range of strain is accentuated to a small extent only for the thin-walled hollow specimens tested in repeated torsion. It may be remarked that there was no evidence at all of buckling in the thin-walled specimens. Specimen A. 2, Table 2, whose wall-thickness was about twice that of the other hollow torsion test-pieces, required a "calculated" range of stress intermediate between those of the latter and of the solid specimens.

The considerably higher ranges of stress, as calculated on the assumption of uniformly varying stress, endured by the solid specimens in torsion (and also in bending), point decisively to the conclusion that the distribution of stress over the section does not correspond with that assumption. It is clear that, as suspected previously,* the Wöhler ranges of stress determined by bending or torsion alternating tests of solid specimens, and calculated from the loading on the above assumption, must be something like 15 per cent. too high.

In order to compare the ranges of strain at or near the fracture point, it is perhaps most fair to take the ratio of these ranges to the ranges that may be assumed would have existed at the respective ranges of stress if the strain had remained purely elastic. The latter are calculated, Table 5 (page 152), from the ranges of strain measured at the small ranges of stress at the commencement of the test, and put in Tables 2, 3, and 4, under the heading of "preliminary dead-weight test." It will be seen from Table 5 that this ratio is only fairly constant. The Author does not as yet infer that a definite minimum range of strain must be established before fracture will occur. The effect of speed upon range of strain requires further investigation, and further tests with very prolonged repetitions of given ranges of stress and their corresponding ranges of strain are wanted. And although a consideration of the latter parts of the tests of A. 2, A. 7, and B. 27 suggests that a very large number of cycles of about $\pm\frac{1}{4}$ ton per square inch less range of stress than the actual range at fracture would have been required to produce fracture, yet it seems probable they would have broken

* Brit. Assoc. Report 1913. Report on Alternating Stress. Page 24.

TABLE 5.

Comparison of Ranges of Strain at or near the time when Specimen cracked.

1. Specimen.	2. Test.	3. Range of Stress when specimen cracked.	4. Range of Elastic Strain corresponding to Range of Strain of Col. 3 = A.	5. Observed Range of Strain at cracking = B.	6. Ratio $= \frac{B}{A}$	7. Number of Repetitions of Range of Col. 3.	8. Remarks.
A. 2 hollow thick wall	Torsion	tons □″ ±6·85	$\frac{7\cdot5 \times 6\cdot85}{6\cdot00} = 8\cdot57$	12·95	1·51	307,000	Number of repetitions at ± 6·60 was 2,230,000.
A. 10 hollow	Torsion	±6·50	$\frac{5\cdot54 \times 6\cdot50}{5\cdot00} = 7\cdot20$	9·75	1·355	1,104,000	
B. 22 hollow	Torsion	±6·25	$\frac{5\cdot86 \times 6\cdot25}{5\cdot00} = 7\cdot33$	10·72	1·46	2,235,000	
B. 27 hollow	Torsion	±6·10	$\frac{6\cdot03 \times 6\cdot10}{5\cdot00} = 7\cdot37$	11·36	1·54	1,037,000	
B. 30 hollow	Torsion	±6·25	$\frac{5\cdot87 \times 6\cdot25}{5\cdot00} = 7\cdot33$	10·65	1·45	940,000	
A. 7 solid	Torsion	* ±7·40	$\frac{7\cdot40 \times 7\cdot00}{5\cdot00} = 10\cdot36$	14·66	1·42	1,527,000	* The range at which fracture occurred was ±7·65, but the number of cycles at this range was only 35,000.
A. 8 hollow	Bending	±13·00	(about) $\frac{7\cdot20 \times 13\cdot00}{12\cdot00} = 7\cdot80$	10·78	1·39	164,000	
A. 14 hollow	Bending	±12·75	$\frac{5\cdot36 \times 12\cdot75}{10\cdot0} = 6\cdot83$	9·00	1·32	550,000	
A. 16 hollow	Bending	±12·75	$\frac{5\cdot13 \times 12\cdot75}{10\cdot0} = 6\cdot55$	8·30	1·27	416,000	
A. 11 solid	Bending	±15·30	$\frac{6\cdot60 \times 14\cdot30}{10\cdot5} = 9\cdot00$	11·97	1·33	301,000	
A. 15 solid	Bending	±13·75	$\frac{6\cdot12 \times 13\cdot75}{10\cdot0} = 8\cdot41$	10·40	1·24	about 270,000	
B. 29 solid	Bending	The ranges of strain at ±14·90 and ±15·60 did not approach a constant value. The ranges of strain were very large (see Table 3), and indicate that for large ranges of stress that can be endured for a small number only of cycles, the ranges of strain are very large.					

under a smaller range of stress with a less range of strain. The curves for the tests suggest that the range of stress that will cause fracture in, say 100 million cycles, will be nearer the range that actually broke the specimens than the range that brought departure from the elastic condition.

The effect of change of speed upon range of strain, illustrated in Figs. 9, 10, and 11 (pages 130–133), has already received some attention from the Author.* The information herein contained confirms the statements already made, and appears to justify the following argument. Considering the curve, Fig. 11 (page 132), for A. 28, the large increases of range of strain at 371,267 and at 475,432 cycles, contingent upon the change of speed from 200 to 2 cycles per minute, may be reasonably attributed to the greater opportunity for plastic movement for the longer cycle. The augmented ranges shrink under continuance of the slow cycles; apparently a process of hardening goes on, resulting in a smaller plastic movement. If after the shrinkage, as at 372,935 cycles, the speed is put back to 200 per minute, there is an immediate contraction in the range of strain, due now to shorter time for plastic movement, but the new range is less than it was before the original increase of speed. The reduced range, however, increases quickly at the restored higher speed. Again, supposing a test, as that of B. 30, to be begun at 2 cycles per minute, or to have continued for a considerable number of cycles at that speed, a change to 200 per minute gives a contraction of strain; continuance at the latter speed increases the range, and the effect of subsequent changes of frequency of cycle is exactly as described above. On the other hand, a change from a given speed and restoration to this speed after a few moments only gives little or no alteration of the range of strain at this speed. In view of these facts, the conclusion seems irresistible that the material, after a considerable number of cycles at a given frequency, does not immediately enter into a cyclic condition suitable to a new frequency; and that the condition of plasticity, or of mobility, is different for different speeds of cycle.

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* Proc. Roy. Soc. A, Vol. 92. "On Speed Effect and Recovery." . . .

If Sir George T. Beilby's hypothesis of the formation of "mobile" material upon the gliding interfaces during crystalline slipping, and of the tendency to hardening of this material, be adopted, the condition of strain at any time during alternate stressing at a given range above the elastic limit may be regarded as the net effect of the opposing processes of production and hardening of the mobile material. As a result, the range of strain would tend towards a value which is not exceeded at that range of stress and frequency of cycle, just as exhibited in the curves of this Paper and in those of Bairstow.*

The above conclusions that the condition of plasticity of mild steel under alternations of a given stress range is a function of the speed of cycle is not easy to test directly. If it is correct, then increase of range of stress should give a greater immediate increase of range of strain when the speed of cycle is 200 than when it is 2 per minute. Unfortunately the Author was unable to measure these immediate increases. An attempt was made in the case of No. A. 10 to obtain some confirmation in the following way. At 1,442,600 cycles, Fig. 9 and 9*a* (page 130), after $\frac{1}{4}$ million cycles at 200 per minute, the machine was stopped and as quickly as possible the same range of stress was applied by dead-weights. Just before stopping the machine the range (at 200) was 9·00; the range with the weights was 11·65, and the hysteresis loop for the cycle was very far from being a closed curve. Again, at 1,443,698 cycles, after 690 cycles at 2 per minute, the range with dead-weights taken as soon as possible after stopping was 10·26, and the hysteresis loop was nearly a closed curve; whereas the range before stopping was 9·90. The range of stress was the same in each case. These results point out to a condition of more mobile plasticity at the higher frequency.

Conclusions.—Mild steel will endure a very large number of repetitions of a range of stress—considerably greater than the range at which the extra-elastic strain appears—that induces a

* Phil. Trans. Roy. Soc. A. 210.

considerable range of extra-elastic strain. The range of stress (calculated on the assumption of uniformly varying stress) which induces fracture is greater for solid than for hollow test specimens.

The effect of giving rest to a specimen in which an extra-elastic range of strain has developed is to reduce the range of the strain. The physical effect of the rest appears to be similar to that of hardening after strain, and not to be of the nature of recovery of elasticity.* The Author has not observed in these experiments (with equal + and − stresses) any real adjustment of elastic limits to range of stress.

There appears to be a marked variation, with frequency of repetition of cycle, of the physical state of mild steel subjected to repetition of a higher range of stress than that consistent with unimpaired elasticity, the mobility being greater with higher frequency.

For the dead mild steel used in this work the range of the induced maximum shear stress at which the elasticity becomes impaired is sensibly the same in both alternating torsion and alternating bending.

The cost of the machine and apparatus used in the experiments have been defrayed by grants from the Royal Society, the Institution of Mechanical Engineers, and the University of Liverpool. The Author is indebted also to Professor W. H. Watkinson, for facilities for carrying on the work in the Engineering Laboratories of the University of Liverpool; and to Mr. C. W. Cook, of Manchester, the builder of the machine used.

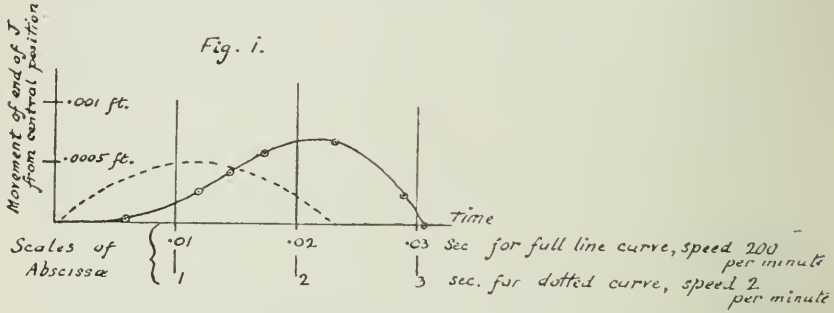
The Paper is illustrated by Plate 3 and 14 Figs. in the letterpress, and is accompanied by an Appendix with 3 Figs.

* Proc. Roy. Soc. A. 92, page 374. "On Speed Effect and Recovery," by the Author.

APPENDIX.

SPEED EFFECT AND INERTIA OF THE MECHANISM.

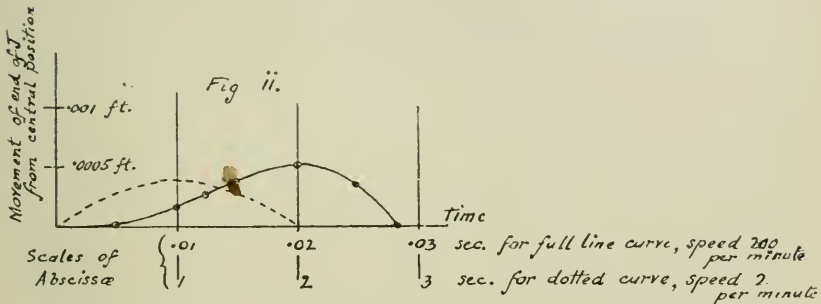
As mentioned on page 125, the range of torque imposed upon a specimen of sensibly perfect elasticity became very slightly increased, and the measured range of strain also proportionately increased when the speed was put up from 2 to 200 cycles per minute. When cyclic non-elastic strain had appeared, the same change of speed caused a *decrease* of range of strain, and the amount of the decrease became continuously greater with the development of non-elastic strain. The question arose whether this effect of speed was in any way a characteristic of the machine merely.



That it is not a result of increase of amplitude of angular twist, giving in some way lift of the springs S, Figs. 2 and 4 (page 122), without imposing the full torque, was shown by the behaviour of specimen B. 31 of the small diameter 0·3125 inch. This was given a large amplitude of *elastic* twist, with the result that there was the percentage increase of the amplitude of about 1 per cent. usual with increase of speed from 2 to 200 per minute. The speed effect cannot be attributed to vibration of the mirror apparatus, or of the specimen. If these vibrated appreciably at the higher speeds, the method of measuring the cyclic strains would not have been possible. There remains the question whether this effect could arise as a consequence of any change of motion of the lever J, due to the character of the forces imposed upon it by the specimen when fatigued.

The motion of the end of the lever J against the resistance of the springs S, is, when the speed of the machine is very slow,

represented by the dotted curve of Fig. i, which is very nearly an arc of a sine curve; and in which the abscissæ are time and the ordinates the amount of the departure of the end of J from its position of rest. The maximum departure—represented by the maximum ordinate of the dotted curve—of course depends on the amplitude of the motion given by the mechanism to the torque lever H, Fig. 5, Plate 3, on the stiffness of the springs S and on the torsional rigidity of the specimen. As stated on page 125, this maximum departure did not exceed $\frac{1}{200}$th inch during the tests recorded in the Paper.



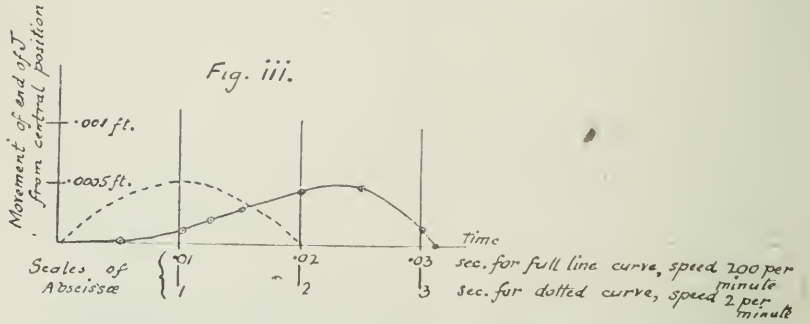
Taking specimen B. 27 as an example, the range of torque was ± 11·6 ft. lb. with a range of strain of 6·58 cm. on the scale, and a lift of J (that is, of the springs S) on either side of 0·0005 feet, the elasticity being sensibly perfect and the speed very slow. A rather tedious calculation shows that with nothing altered except the speed, the inertia of J * will cause the range of torque to become ± 11·8 feet-lb. at 200 cycles per minute, and the lift of J to become 0·0007 foot. The curve of departure (or lift) of J is now the full line curve of Fig. i; but since, in working, the lift is kept as nearly as possible the same amount, the state of affairs is represented by Fig. ii, in which the lift of J is the same as before (that is 0·0005 foot), and the range of torque is shown by the calculation to be ± 11·75 feet-lb. Thus the increase of torque

* The moment of inertia of J about the axis of the test-specimen is 0·057 in engineers' units $\left(\frac{lb}{g}\text{ feet}^2\right)$. The springs S each extend 1 inch under an axial force of 1 lb.

is by calculation 0·15 feet-lb. or 1·3 per cent. when the speed is put up from 2 to 200 per minute and the lift is kept the same; or with a lift of 0·005 inch at both speeds, the increase is 1·1 per cent. Measurements of the increase of range of strain with this change of speed showed agreement between this percentage increase of strain and the percentage increase of torque given by the calculation.

When the material has developed non-elastic strain, then as regards the effect of the inertia of J, the altered rigidity of the specimen must be considered. Assuming for the purpose of calculation that the rigidity is diminished, during increase of torque in the cycle, by 50 per cent., the curve of lift of J is now that of Fig. iii, and the percentage increase of torque for a speed of

Fig. iii.



200 per minute is 0·75. This figure, of course, depends on the above estimate of loss of rigidity; but the important point is that calculation gives still an increase and not a decrease of range of torque when the springs S lift the same amount at the higher as at a very low speed.

Direct evidence that the speed effect is only remotely, if at all, connected with any mere machine effect is afforded by the *rate* of decrease of strain with increase of speed and vice-versâ. The parts of the graph of Fig. 11 (page 132) between 411,580 and 413,955 cycles, and between 416,600 and 419,300 cycles, which are typical of several such curves for different specimens, show that the alteration of range of strain with speed in a "fatigued" specimen is greatest at the lowest speeds, the latter being so slow that no machine or inertia effect could be of importance.

Discussion in London.

On the motion of The President a hearty vote of thanks was accorded to Dr. Mason for his Paper.

Mr. R. G. C. Batson congratulated the Author on overcoming so successfully the many difficulties which presented themselves in constructing a machine to test materials under alternations of combined stress, bending, and torsion, for although the Author had given no results under combined stresses he had stated that the machine was designed for that particular purpose. In his Paper the Author had followed more or less on the lines which were adopted by Mr. Bairstow, of the National Physical Laboratory, in his researches on cyclical variations of stress in iron and steel. Those lines did not give a rapid method of determining the limiting range of stress in a material, but it was very satisfactory to see that the Author found that Guest's Law held for alternating bending and torsion. Unpublished results which had been obtained at the National Physical Laboratory by using an entirely different method from the Author's—a method which had been suggested by Mr. Bairstow as the result of some of his labour on cyclical changes of stress—confirmed the Author's conclusions as regards Guest's Law, and these he had furnished in a written communication (*see* page 182).

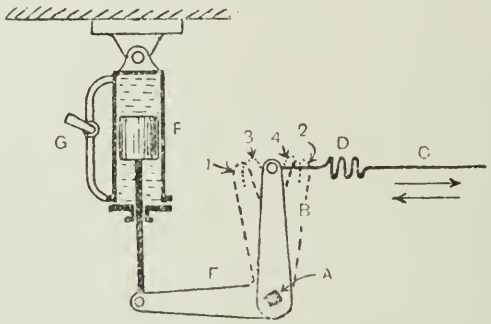
Mr. C. Humphrey Wingfield said he thought a mechanical analogy might be of some interest to the members. The fact that the range of strain became less at rapid alternations might be illustrated mechanically, Fig. 17 (page 160).

The diagram did not represent the whole of the facts by any means, but only under certain considerations. The plasticity was represented by the resistance to motion of a piston in a "cataract." F was a cylinder full of oil, the ends of which were connected by a

(Mr. C. Humphrey Wingfield.)

pipe, the resistance to flow through which was adjustable by a tap G. F was connected by a lever E to one end of the test-piece A, which was square at both ends. At the nearer end of the test-piece there was another lever B, which was pulled backwards and forwards by any spring device, D. If C was moved slowly a definite distance in the direction of the lower arrow, and then back in the opposite direction, the tap being turned off sufficiently to give a compression of the spring, representative of the stress which was applied by torque to the test-piece, the piston F would allow E and B to move a certain distance, representing the amount of strain. If C were moved more rapidly and the motion reversed when

Fig. 17.



the length of the spring was altered by the same amount as before, the oil at first displaced had not time to get round by G, and B would move a less distance than at first. First of all it would move over the range of strain 1–2; in the second experiment it would move only over the range 3–4, although the spring was stressed to the same amount, so that as the speed of reciprocation of C was increased the angle through which B passed (i.e. the amount of strain) was diminished exactly as the Author had found to be the case (*vide* at foot of page 133).

That mechanical analogy failed in that it did not show why the range of strain, with a repeated stress of constant amount and speed of application, diminished. This, as suggested by the Author, on

page 153, was no doubt due to the hardening effect of repeated plastic stress which must be familiar to any one who had bent or twisted wire backwards and forwards, when the stiffening was very perceptible. Captain Sankey's well-known reciprocating hand-testing machine not only showed very distinctly that the specimen when it was bent slowly backwards and forwards hardened, as the Author had expressed it, and became stiffer and more difficult to distort, but measured the increased resistance. (See 8th Alloys Report for instance, Proceedings 1907, pages 282 and 285.) In a Paper comparing methods of testing, of which Captain Sankey was a joint Author with Mr. Bertram Blount and the late Mr. W. G. Kirkaldy (Proceedings 1910), they said: "The effect of the bending is to stiffen the material, and apparently this stiffening tends to a limit." This was very much what the Author had found.

He hoped that the Author would give a rough idea of Sir G. T. Beilby's hypothesis. He was acquainted with his views regarding the flow of solids when their surfaces were polished, but had not met with the Paper referred to on page 154.

The Author's frequent references to Papers read before the Royal Society were most tantalizing. He felt it was a great pity that so much valuable research was buried in the publications of this somewhat Academical Society, which reached comparatively few readers; and where, for all the good it was to engineers who had not access to these Proceedings, it was entirely lost and useless.

The President (Mr. Michael Longridge) said that, as no one else rose to continue the discussion, he might say a few words. He had found the Paper difficult to read, and, even after hearing the Author's clear and comprehensive verbal exposition, he was not sure he fully understood its purpose. He had been much interested by the figures showing the approximate constancy of the strains through large numbers of cycles under stresses nearly approaching the breaking stress, as compared with the much more rapid increases of strains under stresses, but little above the elastic limit of the material. The phenomenon was new to him, and he had hoped that some member would have put forward some theory to

(The President.)

account for it. He understood that the experiments were to be continued. If they were, he hoped the Author would go upon the sound principle of dealing with one variable only at a time. In each of the experiments described in the Paper both speed and stress were varied. No doubt the sudden changes of speed and sudden changes of stress showed phenomena which were of very great interest to science, but practical engineers wanted data to enable them to predict the life of a piece under known stresses. He believed railway engineers had arrived by long experience at the length of time a crank-axle ought to run, and when it had run that time they took it out and had it very carefully examined. In his own experience there was certainly some connexion between stress and life. The higher the stress the shorter the life, and the lower the stress the longer the life, and he was very much inclined to think there was really no limit as to the stress at which a thing would break if the stress was put on a sufficient number of times. He would like to see two series of curves, one stress as abscissæ cycles endured as ordinates, the other cycles per minute as abscissæ cycles endured as ordinates. The magnitude of such a work was discouraging, but its value would be great.

Dr. F. Rogers (Sheffield) thought it was very important to note that the Author had dealt with both the plastic range and also the elastic range of steel. At the same time, experimenters were now pretty well convinced that in the elastic range of stress there was also a hysteresis loop of recoverable strain. One thing that came out from the work, not only of the Author but of others, was that, as research went on, it was found that the stress at which plastic deformations began to show was lower and lower every year, due to the fact that the methods of discovering it had become finer. Thus, whilst the yield-point of the steel in the tensile test was 9·9 tons per square inch, the limiting range in the endurance test was $\pm 5 \cdot 5$ tons per square inch.

He thought all the members would agree with him that the Author had made a good effort in his mechanism to meet all the refinements that could possibly be foreseen at the present time,

in order to get the stress well under control and the means of measuring the strains accurate. He desired to refer to the statement made at the bottom of page 154: "Mild steel will endure a very large number of repetitions of a range of stress—considerably greater than the range at which the extra-elastic strain appears—that induces a considerable range of extra-elastic strain," that is, of permanent strain. That was the case if the steel were a good one. Now the steel dealt with by the Author was a very low carbon-

FIG. 18.—*Steel containing about* 0·15 *per cent. carbon.*

This mild steel is seen to consist of a sort of concrete mixture of iron (the crystalline white ground work) and about $\frac{1}{4}$ as much of comparatively hard steel (0·90 per cent. carbon) in a more or less laminated form.

× 1,000 diameters.

steel; in fact, it was lower than was commonly used for almost anything nowadays except a very few special purposes. Incidentally he was interested in that particular steel because it was one of the British Association steels which he was instrumental in obtaining for his Committee.

A very important point, which possibly to a certain extent the Author had recognized, was that the early stages of the inelastic strain—the strain which occurred when any strain first began to be at all present—was mainly in the ferrite constituent of steel. The members were well aware that the steel was a sort of concrete consisting of about seven parts of ferrite, that is, pure iron, and one part of comparatively hard 0·9 per cent. carbon steel. The diagram, Fig. 18, showed this, the dark areas being the harder steel, and the crystalline white ground the soft iron, as seen magnified 1,000 diameters. He was not quite sure, speaking from

(Dr. F. Rogers.)

memory, whether the Author had used that steel in an annealed condition or not, but from what he definitely knew of the manufacture of that steel, it would not make very much difference whether that particular steel were used in a rolled or an annealed condition in any commonsense manner, so that that point might be dismissed for the moment. The important point to which he wished to return, was that it might be taken that the first inelastic strain occurred in the ferrite, the soft iron, the main component of the steel. Given reasonably good material, it was on that account that the steel would bear the frequent repetitions of the inelastic strain. A parallel was obtained in that case with the good old-fashioned material, best wrought-iron, which was practically entirely pure ferrite (that is, iron). That material might be repeatedly visibly over-strained and yet a wonderfully long life might be obtained from it. That, he thought, was an interesting confirmation, and he brought it to their notice in order to visualize the importance of the structure of the material as it could be seen under the microscope, and in order to show that theory as regards the microstructure was not merely a fanciful one, but a matter of every-day practical utility.

In the next place, he desired to refer to fracture D shown on Plate 3. That fracture seemed to be a very extraordinary one, and in a material of that class it was typical only of what could be obtained by continually varied torsion. He thought it would be very difficult to produce that fracture in any other way, unless the material were in an extraordinarily unusual condition in regard to its heat treatment. It might be taken that the material was in every way normal, because it had been proved not only by the Author's experiments on the steel but also by the results already obtained by one or two other investigators.

Results were also quoted in the Paper of a tensile test upon the material, which showed that it was a very good tough mild steel. He wished to emphasize again that the Author's machine, although it might appear elaborate in a sense, was on the right lines; such elaboration was, he ventured to suggest, inevitable, but it was research.

Dr. Mason, in reply, said he would be extremely glad if Mr. Batson would give the results of the tests to which he had referred. He presumed that the tests had been made on the British Association steel, and he would very much like to know if the stresses and the elastic limits which he found in his own tests were confirmed by those of others.

Mr. Wingfield's mechanical analogy was quite a good illustration, but it broke down at the point to which Mr. Wingfield had himself referred. The mechanical analogy was only exact for the effect of variation of speed; it certainly did not represent the contraction of strain when a low speed was kept constant after a change from a higher speed.

With regard to the President's remark as to working with only one variable at a time, and keeping other factors constant, he started with the resolve to carry out that idea as far as possible, but he was very much surprised to find that the speed effect was so large. When he first came upon the speed effect, he thought there must be some large inertia effect in the machine, and he set about to see where the "leakage" of strain was after he had put the speed down from 200 to 2 per minute. He thought the very first thing to do was to investigate that effect of change of speed, especially that at the particular point where Mr. Wingfield's analogy broke down. He had, indeed, at the outset intended to slow down the machine and to take measurements of the strain throughout the cycles of stress, thinking that there would be a very little difference between the cycle of strain at a low speed and the cycle of strain at a high speed. He wanted to carry out the idea which, as Mr. Batson had already said, was applied by Mr. Bairstow. He might mention that an attempt, though not a very successful one, to do the same sort of thing was made early by Sondericker.* He found, however, that in order to carry out the intention of measuring the strain throughout cycles he had to see what the effect of the variation of speed was; and he therefore thought he had been justified in not going on at once with certain of the main

* Massachusetts Inst. of Technology, Quarterly Journal, 1892 and 1899.

(Dr. Mason.)
objects for which the machine was made, but in investigating the physical state of the steel at different speeds. He thought that was certainly well worth doing.

The method of one variable only had been the method commonly employed in alternating stress tests, and, although he had intended to continue in that well-beaten path, he found that on the whole it was better to diverge from it. The steel was tested in the condition as received from the British Association Committee; he had not heat-treated it in any way. He thanked Dr. Rogers for his appreciative remarks.

Discussion in Manchester, 1st March 1917.

The CHAIRMAN (Mr. Charles Day) said that to his mind the subject of the Paper was of exceeding importance. One of the problems that all those who had to do with designing were required to consider was the stresses which could be permitted on material working under different conditions, whether under static loads or varying loads of uni-direction or alternating direction. It was most interesting to learn that Dr. Mason had spent four years on this work, and they all hoped he would continue it until he arrived at a conclusion, and then be able to come and tell them almost in a sentence what the result was, so that they could take his words away with them, and put them into practice in their shops. He was sure all the members joined with him in wishing the Author entire success in his research work. In conclusion he moved a vote of thanks to Dr. Mason.

Professor A. B. FIELD said that he took very great pleasure in seconding the resolution. The Paper appeared to him to be of great scientific interest. While the problems which would occur immediately to the minds of practical engineers were much more complicated than those considered by Dr. Mason, the only thorough way of working them out was to take the different features one by one, attack the simpler problem first, and then go to the next

item. He thought that if Dr. Mason would proceed with the subject in that way, piecemeal, though the process would be a very long one, they would ultimately get much light upon the behaviour of steel under alternating, and varying, stress distributions.

The Chairman remarked that there had been some hope that the President (Mr. Longridge) would be present that evening. His absence would be regretted by all because the subject was one to which he had given close attention for many years, but unfortunately he was not able to be with them. Many of them were familiar with the reports Mr. Longridge had issued for many years, giving for almost every breakdown he investigated the number of millions of repetitions of stress that had occurred. These reports had been of very great value, and if Dr. Mason was not familiar with them he should get copies; they were very well worth studying. Mr. Stromeyer had written expressing regret at his inability to attend the Meeting, and stating that, as the Manchester Steam Users' Association's experiments on fatigue dealt in part with the same subject as that of Dr. Mason, he had asked his assistant, Mr. H. A. Jones, B.Sc., who conducted most of the experiments, to explain their nature and the relation in which they stood to Dr. Mason's. He (the Chairman) therefore asked Mr. Jones to open the discussion.

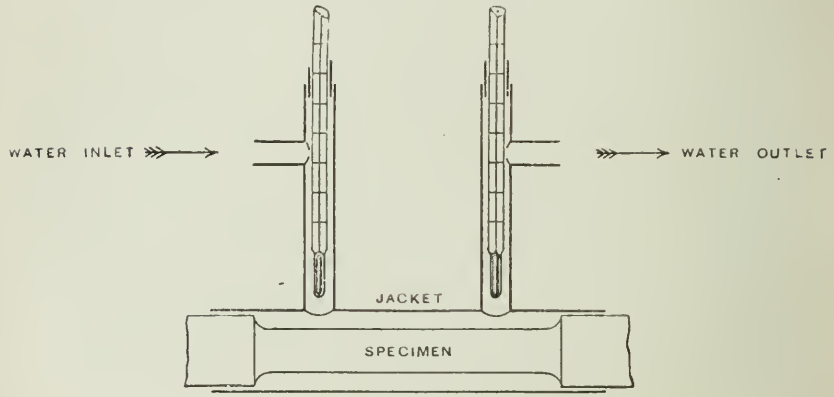
Mr. H. A. Jones said that, as they were all probably aware, Mr. Stromeyer had made many experiments on the fatigue phenomena, and he (the speaker) was fortunate enough to assist him in a large number of them. Mr. Stromeyer had asked him, in his absence, to deal with Dr. Mason's Paper, in the light of their results.

If in the following remarks he seemed to refer a great deal to Mr. Stromeyer's investigations, he hoped the members and Dr. Mason would pardon him, and at the same time accept his assurance that in doing so he was not ignoring the Author's work. His first word was one of appreciation of Dr. Mason's testing

(Mr. H. A. Jones.)

machine. The bending and twisting moments could be definitely measured and their accuracy was not, as in the Manchester Steam Users' Association's machines, dependent on the correct regulation of the speed. Their experiments to a certain extent covered the same ground as those of Dr. Mason, but instead of measuring the width of the strain loop, which was what the Author had done, they had measured calorimetrically the plastic energy given out by the specimen in the form of heat during the fatiguing process under torsional stress, and their respective results ought to be comparable. What they did was to run water through the jacket

Fig. 19.



enclosing the test-piece and to measure the inlet and outlet temperatures, Fig. 19. Other refinements involved the taking of the temperature fall to the grips. Comparisons of tests were, however, somewhat difficult to make, as each experimenter attacked the subject from his own standpoint, with the result that as a rule the conditions differed very widely.

In the present case, all their samples were solid while those of Dr. Mason were, for the most part, hollow. Dr. Mason usually examined each of his specimens under various intensities of plastic stress. They, on the other hand, as far as possible, tested each of their specimens with a constant fatigue stress for the whole time

up to the moment of fracture, having first of all, in each case, determined the fatigue limit which they took to be the stress at which measurable heat was produced. Dr. Mason had confined his researches at present to one quality of material, whereas their tests included a large number of different qualities of steel and other metals.

From the results of the Manchester Steam Users' Association's experiments Mr. Stromeyer deduced that the internal friction H could be expressed by the formula $H = \left(\frac{S - Fl}{Fl}\right)^2 (A + Bt)$, where S was the calculated stress, Fl the fatigue limit, and t the time which corresponded to the number of cycles, A and B being constants which differed for different materials. That, as they would see, indicated a straight line which was part of the curve dealt with by Dr. Mason. For Dr. Mason's steel, if they tried to apply that formula, the coefficient B was very small, for the gradual growth of plastic energy (or plastic strain) was very small as the experiment progressed.

Amongst their samples they had a fair number which behaved in that way, but other specimens appeared to behave quite differently, a number of them showing a steady and considerable increase of heat production as the tests proceeded. From their results, therefore, it appeared that it would not be wise to draw very far-reaching conclusions from Dr. Mason's experiments on a single quality of material, and now that experience had been gained in his method of testing, it would be very desirable to apply it to various qualities of steel and other metals.

Another difference between Dr. Mason's tests and theirs existed in that their stresses for the most part were more severe and consequently produced fracture much sooner than those mentioned in the Paper. Their results appeared to be highly satisfactory as regards determining the fatigue limits, the figure obtained for a dead mild steel being quite comparable with Dr. Mason's results, but they quite failed to establish any rule connecting internal friction and the quality of the material, some of the best and some of the worst producing it steadily or at an increasing rate.

(Mr. H. A. Jones.)

Dr. Mason's experiments were, he hoped, but the beginning of a wider research into the plastic fatigue strains, which would entail an enormous amount of tedious experimenting. The amount of time and patience that Dr. Mason must have already expended on the subject moved him to admiration. Viewing the subject from a purely practical standpoint, he was pleased to see that Dr. Mason added still further corroboration to the existing evidence of there being a definite fatigue limit. While being, of course, deeply interested in plastic stresses from a scientific point of view, engineers had no desire to meet with such stresses in actual practice, and the determination of that limit for different materials should ensure their being able to keep well on the safe side.

With reference to the Author's torsional-strain measuring apparatus, the principle of which was practically the same as that used in the sextant, it might be of interest to remark that that arrangement was applied to statical torsion tests by Mr. Stromeyer as far back as 1882 at University College, London, and the original mirror apparatus was used by Platt and Hayward for their research in 1887.* To illustrate the sensitiveness of that arrangement, he might mention that by using a circular scale of 16 feet radius, Mr. Stromeyer was able to measure the angle of twist corresponding to a stress of 50 lb. per square inch.

Mr. Alfred Saxon said the Author was to be congratulated on the ingenuity and resource displayed in the design of the machine he had had constructed for the carrying out of the tests. All research workers needed encouragement because their task was one requiring a great amount of patience, and accuracy as well, to be of any value, and the Author had shown them that he had exercised that care and patience, and that his tests, as far as they had gone, were of value to engineers. It was only by research work of that kind that they could get to the bedrock upon which they could build in the construction of all the different kinds of machinery that engineers had to deal with. He was quite sure that in these

* Proceedings, Inst.C.E., Vol. xc., p. 408.

days of high speeds, with aeroplane work, and in fact all kinds of work, research of the character Dr. Mason was undertaking would tell very much in the selection of material, and in the best way of utilizing that material. He would, however, like to ask the Author whether he made any real attempt, in connexion with the comparison of the solid and hollow specimens, to get the same kind of sectional area with the hollow test-pieces as he used in connexion with the solid. It did not appear to him to be so at all, and for the purpose of a comparison one would think that the same amount of material ought to be at the disposal of the experimenter in the hollow test-piece as he had arranged for the solid.

In reference to the diagram the Author sketched when speaking about the alteration of speeds and the effects of speeds, he (Mr. Saxon) did not quite follow whether those were confined only to the torsional experiments or whether the effect of speed was dealt with in regard to bending. He would be very pleased indeed to hear the results of the experiments in the combined torsional and bending tests. Dr. Mason had told them that he had only dealt with those problems separately, but the machine was so constructed that he could deal with them combined, and there was so much engineering work subjected at the same time to both those kinds of stresses that the results would be of considerable value.

Mr. W. C. Popplewell, in a written communication which was read by the Chairman, said that the Author, in so ably carrying out the valuable work described, had attacked the question in a manner not employed previously, and in so doing had been able to probe still further into the phenomena of reversed stress and strain. Like Mr. Guest, the Author of the present Paper, in using thin tubular specimens, had avoided one of the possible difficulties caused by the variation of stress at different radii and the incorrect assumption of perfect elasticity in calculating the stress.

One question which struck him (Mr. Popplewell) was whether it would not have been better to secure the mirrors to two points on the parallel portion of the specimen, so as to avoid the strain

(Mr. W. C. Popplewell.)
of the taper portion between the screws and the parallel part? He would also ask why a longer optical lever was not used and thus avoid the double reflection? He (Mr. Popplewell) had constructed clips for holding the mirrors on the parallel portion of a torsion specimen, though those were certainly for use with steady torques. In that connexion Professor B. Hopkinson was able to hold an oscillating mirror on his direct specimens when they were undergoing 7,000 reversals per minute.

The main idea of the Paper appeared to consist of a determination of the relation at every point in each experiment, between the number of reversals up to that point and the range of strain. By following out each run in that way it was possible to watch the change of range of strain, and by noting its variation as dependent on the total number of reversals up to any point, to observe the gradual approach of failure by cracking, and at the same time to note alterations in range of strain produced by variations in speed of reversal, both immediately following a change and for some time afterwards.

By treating the subject in that way Dr. Mason had indicated a way of learning a good deal about the internal phenomena of the mutually related stress and strain in repeated reversals not hitherto known. The main curve on Fig. 8 (page 129) showed that with a plus and minus stress of 6·25, which was apparently sufficient to cause fracture after 2,643,000 reversals at 200 per minute, the range of strain gradually increased up to the point of failure. That was not only interesting, but very important as indicating that as the range of strain increased, the greater became the likelihood of fracture. That was emphasized by the appearance of the curves at the earlier part of the same diagram, where both stresses and ranges of strain were lower.

The portion of the Paper which appeared to suggest promise of results important to engineers in practice was that which bore on the speed effect. That point had been discussed by several investigators in late years, and there had been considerable divergence of opinion which it seemed possible the Author's investigations might help to remove. Up to now the general idea

as to the effect of speed in causing failure had been the following: For a given range of stress the hysteresis loop was wider for slow than for high speeds, which meant that the algebraic sum of the residuary strains was greater also, or, in other words, the positive minus the recovered negative strain was greater. The difference was caused by the longer time-intervals allowed for development of strain. It had further been supposed that where there was greater residuary strain there was greater expectation of early fracture.

The Author's investigations appeared to confirm both these contentions in so far as his range of strain was found to grow when the speed was reduced, and to grow with the approach of failure A number of experiments could be quoted both against and in support of the above contention. Professor Osborne Reynolds and Dr. J. Smith found that higher speeds were conducive to lower breaking stresses, but their speed variation was small—only from 1,337 to 1,917. Against that there were the results of Professor Hopkinson, which showed that the resistance of steel at 7,000 reversals a minute was much higher than at a lower speed, say 1,000. Dr. Stanton, when carrying out endurance tests of rings in which there was a combination of repeated bending and abrasion, found no evidence of speed effect from 100 to 2,200 reversals per minute. Experiments at University College on rotating beams showed that up to a speed of 1,300 reversals per minute there was no evidence of speed effect. The writer found also no speed effect with extremely slow reversals. That experimental evidence appeared to indicate that, until a speed of several thousand reversals a minute was reached, the time effect did not come into play. He mentioned these facts and opinions as showing that there was considerable difference of opinion as to the effect of speed on endurance, but that the balance was so far on the side of increased endurance with higher speeds.

It was interesting to learn from the Paper that Dr. Mason was of opinion that rotating beam experiments gave results which were too high, and that he further indicated the excess as 15 per cent. He (Mr. Popplewell) had found that general effect to exist, but if the beams were solid in most cases the discrepancy was more than

(Mr. W. C. Popplewell.)

15 per cent. In any case, it was very satisfactory to have further evidence from the latest experiments, carried out on quite new lines, that the usual method of making the calculations of stress in rotating beam experiments, on the assumption of perfect elasticity throughout, was unsound.

Though the Author had penetrated a long way towards explanation of the sub-mechanics of reversals of stress, it was a pity he was not able, in addition, to run a number of simple endurance experiments from rest to fracture at different speeds, as they would have provided new and definite evidence of speed effect at the speeds used. Those would have appealed to the people most directly concerned. The whole of the Paper was full of confirmation of the fact that the phenomena of stress and strain during reversals of stress were intimately bound up with "time," and it was especially important to recognize that when considering the speed effect, which was apparently wholly dependent on the speed with which semi-plastic strain followed stress.

Mr. Herbert Carrington said that he would like the Author to say a few words further concerning the position of the elastic limit. Reference to Figs. 10, 12, 13, and 14 (pp. 131–148) would show that, during a period of stress reversals of constant intensity, the width of the hysteresis loop gradually increased. This would indicate that either the curved ends of the loop became more blunt or that the position of the elastic limit gradually decreased. Such a possible reduction in the position of the elastic limit would be supported by an investigation of Mr. Popplewell,* who showed that the application of a few slow reversals of stress of constant intensity of an amount slightly greater than the primitive elastic limit resulted in the formation of a constant artificial limit which was in some cases considerably less than the primitive.

It would also be interesting to know whether the Author had noted any variation in the value of Young's Modulus for stresses within the elastic range during the life of any one specimen. The

* Proceedings, Inst.C.E. 1914.

fact that the strains gradually increased during the application of stress reversals of constant intensity indicated a decrease in the slope of the hysteresis loop, and hence suggested a decrease in the value of Young's Modulus. Further reference to the Figs., however, would show that these increasing strains were approximately proportional to the corresponding increasing width of the hysteresis loop, which would appear to indicate that the value of Young's Modulus was approximately constant.

Professor A. B. Field said that one speaker had mentioned that they did not want to incur, in practice, strains beyond the elastic limit; undoubtedly they did not want to, but they did sometimes incur them, and it was no use shutting their eyes to the fact. On the subject before them, there was a great deal the practical engineer wanted to know, and he would naturally scan a Paper like the present one to get what he could out of it; in fact, a number of problems would immediately occur to him upon which he would like to get light from Dr. Mason's work.

For himself, two instances came to mind. In the case of high-speed steam-turbine-driven apparatus, where high stresses were necessarily and knowingly incurred, they frequently got high local stresses by reason of secondary effects—due, perhaps, to the superposition of bending upon a direct tension, or in other ways. The stresses as calculated in the designing room or drawing office were based upon simple hypothetical conditions, and could not always take full account of effects introduced by temperature changes, faulty construction, etc., which varied greatly between different machines. They all knew they could not get perfect workmanship. It would be quite foolish to imagine that the machines operated under the ideal conditions which were assumed in the calculations, and there was no question at all but that in the actual machine local stresses were frequently encountered which went beyond the elastic limit. Sometimes they could make a fairly good guess at the kind of effect which might possibly arise, while in other cases high stresses were incurred in ignorance. The question that one would like to ask was: How seriously must one

(Professor A. B. Field.)

view such cases? Sometimes the total number of reversals of stress involved during the useful lifetime of the apparatus might not exceed perhaps a total of 10,000. For instance, he would take the case in which these stresses arose on the starting of the machine and then subsided on stopping it, and he referred to apparatus which would be started and stopped perhaps once in twenty-four hours, or less frequently; so that a life of fifteen to twenty years might only represent some 2,000 to 8,000 cycles.

Dr. Mason had pointed out that in some of his tests the life of a specimen, even after the first hair-crack had developed, amounted to something like 10,000 to 40,000 cycles. That was a very striking result. Perhaps it accounted for the fact that the high stresses which occasionally could be proved to arise in apparatus that had been working for a long time, did not have the serious effects which might have been anticipated. But, on the other hand, they must remember that Dr. Mason had been working with a dead-mild steel, and presumably had eliminated all machining marks from his specimen. This raised the second point—a query as to the degree of evil effect produced by tool-marks; these were the remnants of a surface tearing of the material, and therefore must be expected to form very good starting-points for the first crack of a fracture. Little was really known as to the importance or otherwise of eliminating those tool-marks. It was a result of practical experience that they were apt to pay a great deal more attention to the very careful filleting and smooth finishing of alloy steel than of the ordinary lower grades of material, although the alloy steel was usually a much more ductile material, and therefore, from that point of view, might be expected to be less susceptible to those evil effects. The practice referred to was probably perfectly sound, and brought about by other reasons.

Turning to the actual instrumental part of the Paper and the experimental results, he felt that Dr. Mason was to be congratulated upon the simple and effective type of machine which he had developed. An apparatus designed by Professor B. Hopkinson some years ago was extremely pretty; it involved placing the specimen between two large masses and applying an alternating magnetic

force to one of these masses, the frequency of the force being adjusted to give conditions of resonance for longitudinal extension and compression vibrations. In that way the small force of the electro-magnet was multiplied many times and gave a large stress. The method was used for frequencies up to 7,000 or 8,000 per minute, and gave most interesting results. In those experiments, he believed, Professor Hopkinson was in most cases obliged to measure the strain only and deduce the stress from the strain. In a few cases he made direct measurement of the acceleration of one of the masses, and in that way was able to check the stress; but the apparatus did not lend itself particularly well to that, and therefore was more suited to investigations within, or only slightly outside, the elastic range.

Dr. Mason, on the other hand, had confined his attention to low-frequency investigations, and had thus been able to measure with sufficient accuracy both the stress and the strain. He had brought out clearly the relations between time and range of strain, with a given stress range; also the effect upon the strain range of a variation of frequency between 2 and 200 per minute. These results appeared to the speaker to be of great interest; he would like Dr. Mason to indicate whether he had reason to think that 200 per minute was nearly equivalent to a high frequency, or whether he anticipated a continuance, much beyond 200, of the variations found.

The way in which the behaviour of the material under static tests, applied immediately after cycles of reversals within given stress limits, varied according to the frequency of the previous reversals, was most noteworthy, and the Author's deductions in this connexion on pages 153–4 were very interesting.

Dr. Mason said Mr. Day's very kind remarks ended in this very practical and pointed conclusion, that he would like to have a result which could be put to immediate use in practice. Certainly if the fruits of intermittent labours extending over four years could be wrapped up in a formula, and enshrined in the pages of Molesworth, he would be very glad; but the fruits grew very

(Dr. William Mason.)
slowly indeed, and it was necessary to exercise the care and patience of which Mr. Saxon spoke so feelingly in order to amass even a small total of practical results.

In the communication from Mr. Stromeyer, given so lucidly by Mr. Jones, there were one or two points which interested him very much. Mr. Stromeyer took as the fatigue limit the particular stress at which there was a measurable evolution of heat. To what point in his (Dr. Mason's) tests would that correspond? For the last two million or two and a half million revolutions of the test given on page 129, the piece was warm to the touch. The evolution of heat must have been observed by Mr. Stromeyer at a point very much lower down than the higher part of the curve. He had found that a piece could stand five million repetitions of stress with a considerable evolution of heat. Such a condition must be considerably removed from the point that Mr. Stromeyer observed with his delicate apparatus, and which Mr. Stromeyer assumed to correspond with the fatigue limit. It seemed to him to be probable that the fatigue limit would be higher than the elastic limit. He imagined that the point in Mr. Stromeyer's experiments when the evolution of heat was first noticed would correspond to the elastic limit—that is to say, the limit where the ranges of strain were no longer proportional to the range of stresses. Until further experiments had been done, Mr. Stromeyer's fatigue limit, as determined thermally, and what he (Dr. Mason) believed to be the fatigue limit, differed considerably.

He had seen and admired Mr. Stromeyer's apparatus, and very valuable results had certainly been obtained by it. He himself attacked the problem from a different point of view and, as he thought, with no less delicacy of experimentation, and he should like at some future time to be able to come into agreement with Mr. Stromeyer, or Mr. Stromeyer with him, upon the fatigue limit.

Mr. Popplewell's communication called for some comment. With regard to the hollow specimens, he wished to have them with thinner tube-walls than those which were actually used, although those were fairly thin. Some observations which were made had not been recorded in the Paper, because he thought there was

enough in it without including matter which not immediately related to the point he desired to bring out. From those observations he found that the stress could not be considered as being uniform even over so thin a tube-wall as he experimented with, although of course making them as thin as he had done had been of advantage.

Mr. Popplewell criticized in a suggestive way the construction of the mirror apparatus. Of course his method had certain advantages. He thought they would agree it was desirable to have a solid job. If he fixed the mirrors on to the parallel part of the specimen instead of bolting them as he had done to the ends, then stresses would be introduced in the parallel part, which was one of the things he particularly wished to avoid. Really he did not see much point in strictly confining the observations of strain to the parallel part. The ends of the parallel part had a very gently varying fillet, and it was unlikely that they would be unduly affected by non-elastic strain; even if they were somewhat affected, that would not invalidate any of the work which was set forth in the Paper.

With regard to the speed effect, Mr. Popplewell had made certain experiments at very slow speeds, slower (he understood) even than the two cycles per minute. He would have liked Mr. Popplewell to make some observations on the speed effect, considered on page 153, which had been revealed by the experiments. He agreed in the main with what Mr. Popplewell said about the speed effect which he found at his very low speeds.

Mr. Saxon remarked that it was always desirable to get to the bedrock. He had confined himself to one particular steel for two reasons; first, because one steel at least should be explored very thoroughly; and secondly, because it was desirable to use the steel provided by the British Association. The steel he had used had been the only kind available from that source. It was an advantage that all experimenters should use the same kind of material so that the results could be correlated.

He did not attach much importance to keeping the same sectional area in the hollow and solid specimens. In the hollow ones that were exhibited that evening the dimensions of a crystal

(Dr. William Mason.)
were quite small in comparison with the smallest thickness of tube-wall. In answer to Mr. Saxon's question, both the bending and the torsion gave exactly the same results in every way.

Mr. Carrington asked about the variation of Young's Modulus. Of course that was a very difficult, almost a hopeless thing to determine when the strains were other than elastic, and on that account he had not attempted to do anything of that kind. With regard to the new elastic limits which were supposed to come into being during alternating stress, all he had to say was that in this particular steel he had not found them. It might be a little premature to say so, but he was beginning to doubt whether those new elastic limits did actually come into being. The more delicate the strain measuring apparatus, as it seemed to him, the more remote those induced elastic limits appeared to become.

Professor Field referred to high-speed turbine-work. He (Dr. Mason) was inclined to think—it was only a personal opinion—that where a shaft or anything else broke down or cracked at a strain which was evidently below the ascertained elastic limit, there must have been somewhere stresses beyond the elastic limit induced locally. Dr. Stanton had put into torsion a shaft with a keyway cut into it. Of course he found that cutting a keyway into the shaft diminished the endurance of the shaft enormously. It seemed to him that there must be, in the cases mentioned, some induced stresses that would not matter at all if the stress was not repeated, but if the stress was repeated then the cause of a breakdown was not very far to seek. As for Professor Field's remarks about 10,000 repetitions of stress causing breakdown, it seemed to him that some effort ought to be made to obtain an idea of what the stresses were. The stresses certainly must be far greater than those contemplated in the design and intended to be borne.

With regard to the evil effect of tool-marks, he did not know whether Professor Field was acquainted with the work of Kommers,* who found that, by a final polishing of the surface of a

* Repeated Stress Testing, International Association for Testing Materials, 1912.

specimen, its endurance would be very largely increased. There was no doubt that fracture was much more easily compassed when there was an initial tear produced during machining.

He was not quite sure whether Professor Field supposed that in changing from 200 to 2 revolutions the machine was stopped dead. In changing the speed the machine was not really brought to a standstill.

In reply, also, to Professor Field, he had to say that he had no reason to think that a frequency of 200 per minute was equivalent in its effects to a high frequency. On the contrary, from some later work, he had reason to believe that there would be considerable and continuous difference as between 200 and 800 per minute. In the high-speed experiments of Professor Hopkinson, referred to by Professor Field, the range of extra-elastic strain appeared to have been only about $\frac{1}{15}$th of the elastic strain.

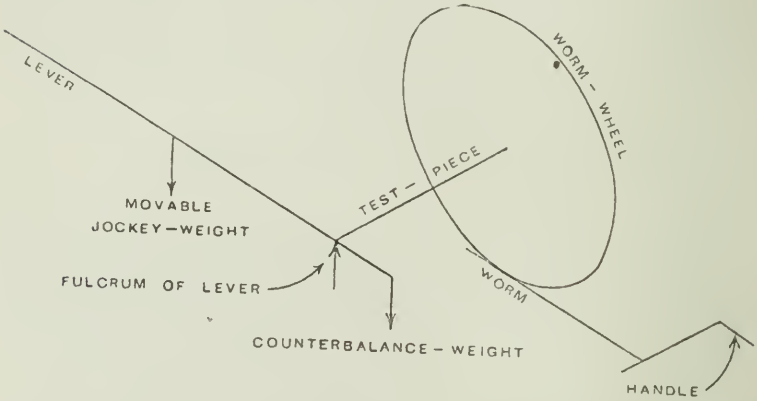
In conclusion, he thanked the Chairman and the members for the cordial way in which they had received his Paper.

The Chairman said an interesting point was brought to his mind by the reference to the effect produced by cutting-tool marks. Small valves were made for aero-engines run at high speeds with the stems drilled down for lightness. The life of the valves was multiplied by at least six, in many cases far more, by carefully reamering these holes so as to remove the drill marks, and by tapering the ends of the hole off very gradually. Another experience was with connecting-rods for aero-engines; the life was multiplied by several times by polishing carefully all over.

Communications.

Mr. R. G. C. Batson wrote, in continuation of his remarks at the Meeting, that during the course of some preliminary experiments in connexion with the "determination of the Fatigue Resistance of Mild Steel under various conditions of Stress Distribution,"* results were obtained which confirmed Dr. Mason's conclusions (pages 149 and 155) that Guest's criterion of elastic failure held under both alternating torsion and alternating bending. The experiments

Fig. 20.—*Diagrammatic Sketch of Torsion Machine.*



were made under alternating torsion on an ordinary static torsion machine. The object of the experiments was to determine the fatigue range in torsion for comparison with that determined, at high speeds, in the special machine * constructed at the National Physical Laboratory for the purpose. The static torsion machine used was of the standard lever type made by Messrs. J. Buckton & Co., of Leeds, the torque being applied by a worm-wheel and measured by means of a lever with a moving jockey-weight as shown diagrammatically in Fig. 20. In order to apply torque in both directions without adjustment of the counterbalance weight, the

* Brit. Assoc. Report 1906. "Complex Stress Distribution."

lever was balanced with the jockey-weight in the centre of its run as shown. The torsional strains were measured optically, the mirrors and attachments of a Martens mirror extensometer being fixed to the parallel part of the test-piece, Fig. 21, by means of two small clips, each containing two pointed set-screws. The distance between the attachments was four inches. The twist of each mirror within the yield-stress was small and was measured by the movement of an illuminated scale relative to the cross-wires of a telescope. The method of measuring the movement of the two mirrors, by the use of a single telescope, has been previously described.* The difference between the readings from each mirror at small increments of the torque gave, when multiplied by a

Fig. 21.

Arrangement of Mirrors for measuring Torsional Strain.

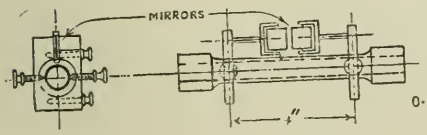
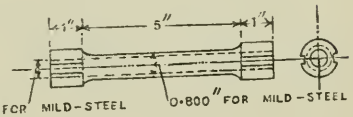


Fig. 22.

Torsion Test-piece.



constant depending on the scale distance, the angle of twist for that increment on a length of four inches. The test-piece, Fig. 22, was hollow (outside diameter being 0·800 inch and inside diameter 0·575 inch for mild steel), and was attached to the lever and worm-wheel by keys and keyways. Three materials were used for the tests:—

(I) A mild-steel provided by the British Association Complex Stress Committee in bars $1\frac{5}{16}$ inch diameter. (Carbon 0·132 per cent.)

(II) A mild-steel of slightly higher ultimate stress in bars of $1\frac{1}{4}$ inch diameter. (Carbon 0·270 per cent.)

(III) High-Carbon Steel. (Carbon 0·654 per cent.)

* International Engineering Congress. San Francisco 1915. "Testing of Materials," Appendix I.

(Mr. R. G. C. Batson.)

The materials were tested in the condition in which they were received, and the results of the tensile and torsion tests on them were given in Tables 6 and 7.

Mr. Bairstow * in his experiments found that for direct tension and compression

(1) The natural elastic range of the material was identical with the Wöhler safe range.

TABLE 6.—*Tensile Tests.*

Steel No.	"Primitive" Elastic Limit.	Yield Stress.	Ultimate Stress.
	lb. per sq. in.	lb. per sq. in.	lb. per sq. in.
I	28,700	30,200	50,100
II	30,000	39,100	65,800
III	—	69,900	118,900
Steel No.	Extension. $\frac{\text{Length}}{\text{Diameter}} = 3{\cdot}5$.	Reduction of Area.	Modulus of Elasticity.
	per cent.	per cent.	lb. per sq. in.
I	49·0	70·8	$29{\cdot}7 \times 10^6$
II	42·5	65·8	$29{\cdot}4 \times 10^6$
III	22·5	43·8	—

TABLE 7.—*Torsion Tests.* (*Hollow Test-Pieces.*)

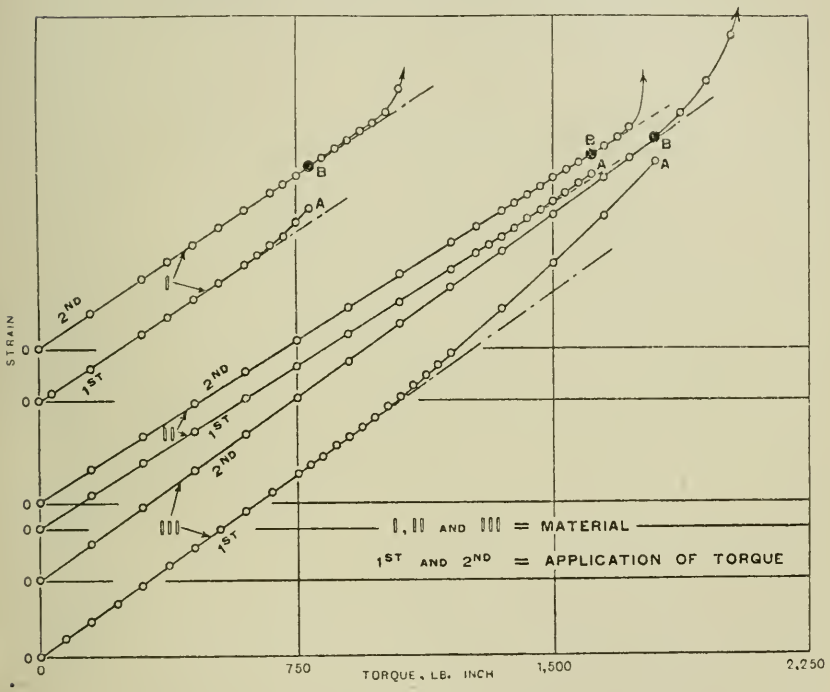
Steel No.	"Primitive" Elastic Limit.	Yield Stress.	Modulus of Rigidity.
	lb. per sq. in.	lb. per sq. in.	lb. per sq. in.
I	18,100	18,800	$11{\cdot}8 \times 10^6$
II	20,400	22,600	$11{\cdot}8 \times 10^6$
III	25,800	—	$11{\cdot}9 \times 10^6$

* "Elastic limits of Iron and Steel under Cyclical variations of Stress." Phil. Trans. Royal Society, London, Vol. 210.

(2) The range of stress between the superior and inferior elastic limits in iron and steel was the same in magnitude

Fig. 23.—*Showing Application of Torque on 3 Materials, the Tests on 2nd Application having been boiled in each case.*

Note.—For each Material, Max. Torque for 1st Application, A, equals Torque at Elastic Limit of 2nd Application, B.



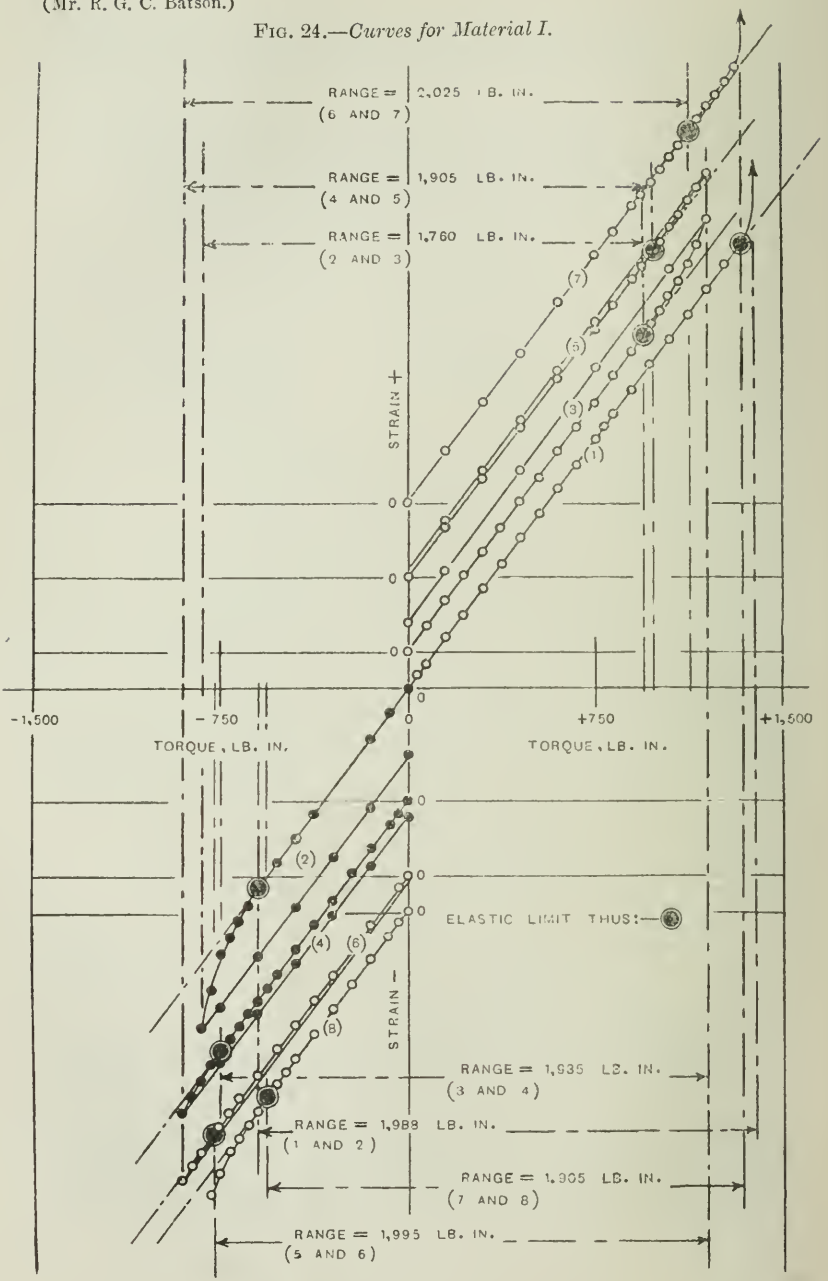
as the maximum range of stress which could be repeated without limit in a specimen of the same material without causing fracture.

(3) The elastic after-working could be eliminated, as suggested by Muir, by immersing in boiling water for half-an-hour.

The method of test for alternate torsion was, therefore, to determine the range of stress between the superior and inferior elastic limit, that is, between the elastic limit when twisted in one

(Mr. R. G. C. Batson.)

FIG. 24.—*Curves for Material I.*

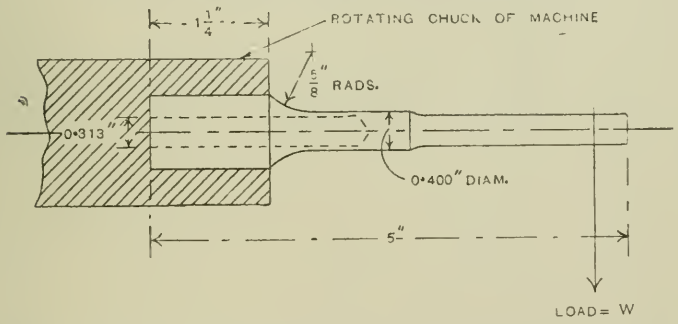


direction and that obtained by twisting in the other direction. Then, assuming that what Mr. Bairstow found for mild steel in tension and compression held for torsion, the range thus found would be equal to the safe range in alternating torsion for the material.

If a tensile test-piece were strained slightly beyond its "primitive" elastic limit and, after releasing the load, boiled in water for 30 minutes, then, upon being retested, the new elastic limit was equal to the stress to which the material had been previously carried.

Some experiments were first made to determine if the same conditions held under torsion. The torsion results obtained were

FIG. 25.—"*Wöhler*" *Alternating Bending Test-piece.*



plotted in Fig. 23, and showed that what was true for tension was equally true for torsion. It followed, therefore, that if a test-piece were twisted in one direction beyond its elastic limit to a shear stress equal to f_{s1} and the torque released; then, if the test-piece were boiled in water for 30 minutes and stressed in the opposite direction giving a shear elastic limit in that direction of f_{s2}, the range of stress between the elastic limits in the two directions was equal to $f_{s1} + f_{s2}$.

This test was carried out on the three materials mentioned above; the alternations and consequent boilings being repeated from five to eight times on each test-piece. The results of these tests were given in Table 8 (page 188) and Fig. 24.

TABLE 8.

Alternating Torsion and Alternating Bending Tests.

Steel No.	Alternating Torsion Tests.				Wöhler Test. (Alternate Bending.) Limiting Range of Maximum Shear.
	Dimensions of Test-Piece.		Elastic Range of Twisting Moment.	Natural Elastic Range of Stress. Maximum Shear.	
	Outside Diameter.	Inside Diameter.			
	inch.	inch.	lb.-in.	lb. per sq. in.	lb. per sq. in.
I	0·800	0·575	1,988 1,755 1,935 1,905 1,995 2,025 1,905 } 1,950	±13,200	±12,500
II	0·800	0·575	2,565 2,400 2,550 2,453 2,520 2,475 2,453 } 2,490	±16,900	±16,200
III	0·750	0·562	2,625 2,625 2,625 2,520 2,685 } 2,620	±22,500	±24,800

The tensile fatigue ranges for the same materials were also found using the Wöhler cantilever method, and hollow specimens, Fig. 25 (page 187), of 0·400 inch outside diameter and 0·313 inch inside diameter. The results of these tests were also given in Table 8.

The agreement in the values of maximum shear stress under alternating torsion and alternating bending was very close, and suggested that the shear stress was the prime cause of elastic failure in both cases.

Dr. Bernard P. Haigh (Royal Naval College, Greenwich) wrote that the Author was to be congratulated on the design of a machine, by means of which, given a moderate frequency of alternation, the ranges of stress applied to the specimen could be measured with comparative accuracy. The machine described was one of the rather small group of alternating stress testing-machines in which the results were obtained in terms of measured stresses and not merely as empirical comparisons. It was noteworthy that the Author's experiments indicated that fatigue occurred in this mild steel when an ascertainable limit was exceeded by the shear stress, independently of whether that was produced in the material by torsion or bending. Thus Guest's approximate law—that ductile yield in such metals occurred when the shear stress reached a limiting value—could be extended to fatigue limiting stresses.

Some experiments made by the writer, also on the British Association mild steel containing 0·12 per cent. carbon, indicated that the same limiting stress was applicable for tests under direct pull and push of equal intensities, applied with a frequency of 2000 cycles per minute. The sample supplied was in the form of a $\frac{9}{16}$-inch round bar and gave the following tensile tests:— Yield stress 22·4 tons per square inch; ultimate stress 25·2 tons per square inch; percentage elongation 17·0 + 77·5 times D/L, corresponding to 24·7 per cent. on specimens of the dimensions tested by the Author. Thus the sample was somewhat stronger and less ductile than in the form of the $\frac{15}{16}$-inch square bars. Fatigue tests were made on 16 specimens and could be summarized as follows:—

(Dr. Bernard P. Haigh.)

$15\frac{1}{2}$	tons per	square inch	..	0·1	million cycles to fracture.
15	,,	,,	..	0·5	,, ,,
$14\frac{1}{2}$	,,	,,	..	1·0	,, ,,
13·2	,,	,,	..	6·0	,, ,,

Taking the endurance of one million cycles for comparison with the Author's results, it appeared that the ratio between the limiting stress and the ultimate strength was $(14\cdot5 \div 25\cdot2) = 0\cdot575$. This was a somewhat high value for such steel, and, judging from experience with similar mild steels, it was probable that the value would have been reduced to 0·55 or even less if the metal had been heat-treated to bring its yield and ultimate strength back to the figures quoted by the Author. On this basis, the limiting stress producing fatigue after one million cycles would have been $(23\cdot5 \div 0\cdot55) = 12\cdot9$ tons per square inch, a value which agreed very closely with those given by the Author for the bending tests, and double those obtained for the torsion tests. It was satisfactory to note such close agreement between the results of very different methods of testing, particularly under alternating stress. In the writer's machine, built and at work in the Engineering Laboratory of the Royal Naval College, Greenwich, under the direction of Professor Henderson, the alternating stress was produced by means of alternating electro-magnets. Means were provided for eliminating all inertia stresses and the stress applied to the specimen followed an almost pure sine wave between equal (or other predetermined) intensities of direct pull and push.

The evidence, given by the Author, that such steel would withstand very great numbers of repetitions of stresses materially in excess of the apparent elastic limit was entirely in agreement with observations made by the writer. It did not seem that a low elastic limit, if accompanied by a fair yield and ultimate strength, was any indication that metal was particularly liable to fail under alternating stress. This applied to brasses as well as to steel. It followed that the relation shown between the fatigue limiting stresses under torsion and direct pull and push was a special effect, and not one that could fairly have been anticipated from Guest's approximate law.

In spite of the interesting variations of hysteresis, produced by altering the frequency of the stress cycle at moderate speeds, there still appeared to be no clear evidence of any change in the fatigue limiting stresses produced by similar changes of frequency. On the contrary, it rather appeared that the widths of the hysteresis loops obtained with high and low frequencies gradually settled down to about the same values. The large effects that had sometimes been recorded as due to variations of frequency seemed rather to be attributable to the methods employed for measuring the stress. It was notable that such speed effects were generally present in the noisier types of machine.

It was probable that the hysteresis effects recorded were characteristic only of the particular steel tested, since the hysteresis found in slow cycles of stress varied so widely in different steels and other metals. An extension of the Author's experiments to Muntz metal or Naval brass would be of considerable interest since the character of fatigue in that metal was substantially different from that in steel. Whereas steels approximately obeyed the law expressed by "Gerber's parabola," this was not so for Muntz, which more closely followed the law given by Goodman's linear equation, when tested under alternating pull and push between unequal limits. It would be advantageous if investigators making such experiments, which possessed a philosophic as well as a practical importance, would choose such metals as possessed characteristic microstructures, for example, ingot iron, pure alpha and composite alpha and beta brasses. The mild steel used by the Author was a good choice in this respect, and was well known in many laboratories.

Dr. W. H. Hatfield (Brown-Firth Research Laboratory) wrote congratulating Dr. Mason on the presentation of the results of his extremely valuable experiments. He (Dr. Hatfield) had the pleasure of seeing the tests in operation, and had naturally looked forward with considerable interest to the publication of this Paper. The method of measuring the stress applied and strain produced adopted in the experiments appeared to be quite satisfactory, and

(Dr. W. H. Hatfield.)
the amount of care and patience which Dr. Mason has bestowed upon his experiments, and which this type of test invariably demanded, would, he was sure, be appreciated by the members of the Institution. It was unfortunate that this kind of investigation must necessarily be spread over a very long time before any very general conclusions could be deduced. This field of investigation was extremely important, and it was to be hoped that a good deal more work on these lines would be put in hand. Unfortunately, there yet remained to be found a shorter method of finding the "life" of material under varying ranges of stress, which would indicate exactly what range of stress could be safely applied for an indefinite period to a given material. Dr. Mason would seem to claim to have established that this range was greater than the elastic range. He would appear to indicate that small amounts of plastic work may be done upon the material for indefinite periods without causing rupture. This would seem to be an extremely important point if it could be firmly established, since the idea has been greatly gaining ground that whilst materials might be stressed indefinitely within their elastic limits, rupture was certain under alternating stress, if the elastic range were exceeded. He would like to ask Dr. Mason whether he considered his elastic range of stress to synchronize with the stress at which the strain ceased to be proportional to the stress. Many metallurgists held that if the elastic range were exceeded, even in a single crystal, alternating stresses of such order would finally produce local rupture, resulting in ultimate fracture. The "speed" effect on the range of "strain" produced indicated that of the two types of action "slip" and "recovery," each of which depended on the time permitted for such action, the first increased most with the lower speed of alternation. This would point to the material responding differently to stresses of different orders of speed of application, for which point of view there seemed to be accumulating evidence. The "stiffening" up, which appeared to take place when a slower speed of alternation was applied, seemed to the Author to provide an argument in favour of Sir George Beilby's hypothesis of a "mobile phase." This was of considerable interest, and the experiments would

no doubt be carefully considered by metallurgists from this standpoint.

The nature of the commencement of failure of the torsion pieces, and the coincidence of the values of shearing stress occurring at the limiting safe loads in bending and torsion, should prove useful to engineers as strongly supporting Guest's theory.

With regard to the position of the cracks developed in the torsion test, he would like to ask Dr. Mason whether he had considered the possibility of the microstructure assisting in this type of fracture. For instance, it would no doubt be well known to him that, in material such as he was dealing with, streaks of sulphide and impurities in the steel occurred, which in themselves might produce or assist the preliminary crack, and it should also be remembered that if the impurities themselves were not sufficiently important, their presence produced a lamination of structure which really ought to be considered. It would be interesting to know the type of fracture produced in a test-piece cut across the grain. If Dr. Mason had the time and facilities for putting through such tests, the Brown-Firth Laboratory would be pleased to supply suitable material.

The Author was to be congratulated upon the beautifully designed hollow test-piece for torsion. In connexion with these pieces, it would be interesting to know whether he had assured himself, and, if so, how, that the test-pieces had not been strained in machining. It was an extremely difficult matter to produce from mild steel pieces with walls as thin as $\frac{1}{40}$-inch, as the initial straining would, of course, tend to vitiate any results obtained.

Dr. Mason wrote that Mr. Batson's communication was interesting and valuable. It appeared to give good reason for believing that the Wöhler safe range could be found by the method of alternate stressing and boiling. In this method the point to which the stress was carried was no doubt determined by an extensometer or equivalent apparatus. It was no easy matter to tell when the elastic limit was reached, and stressing to slightly above the elastic limit was a matter involving delicacy of extensometer and a personal

(Dr. Mason.)
estimate. Also the Wöhler safe range, as determined in an alternate testing machine, was a matter of estimation by extrapolation from the curve of f and n, where these denoted respectively the range of stress and the number of cycles required to produce fracture. It seemed to the Author that the agreement between the range determined by alternate stressing and boiling and the Wöhler safe range was not conclusive evidence that either range coincided with a "natural" elastic range; the idea of Bauschinger that the Wöhler safe range must be a range between some elastic limits had to be imported. That idea was not based on direct experimental evidence. The Author had found his elastic limits by measurement of the cyclic strains during alternate stressing. He had found no evidence of any alteration of elastic limits during long periods and large numbers of repeated cycles. He had found, moreover, that a piece would stand millions of cycles under a range of stress considerably greater than the elastic-limit range with an accompanying large value of extra-elastic cyclic strain. He therefore hesitated, pending further investigation, to subscribe to the doctrine that the Wöhler safe range—that is, the range at which a piece would break with no less than unlimited repetitions—must be a range between elastic limits, "natural" or otherwise.

The Author was very glad to have confirmation of some of his results from Dr. Haigh, particularly the one that the steel used would withstand very great numbers of repetitions of stresses materially in excess of the elastic limits. With regard to the relation between the fatigue limiting stresses in torsion and in bending, the Author had not attempted to draw any conclusions from his own work as to whether Guest's law could have application; an application, as Dr. Haigh pointed out, would be an extension to limiting fatigue stresses of the relation which the Author had found for the range of stresses at the elastic limits under alternating stress.

As Dr. Haigh had remarked, there was no clear evidence of change of fatigue limiting stresses with speed, though the variation of hysteresis was considerable. This was consistent with the result of the experiments of Eden, Rose, and Cunningham. Dr. Haigh

remarked that the ranges of strain at different speeds settled down to about the same value. That appeared to be the case from Figs. 9, 9A, and 11 (pages 130 and 132). The importance of the hysteresis changes lay in the inference that there appeared to be a lower plastic resistance at high than at low frequency of cycle. It remained to be seen, moreover, whether the ranges of strain during the life of a specimen tested at a constant speed, say, of 2 cycles per minute, would be equal to the ranges, at similar stages of life, of another similar specimen tested, say, at a constant speed of 200 per minute, the range of stress being the same for each. This point was under investigation.

He desired to thank Dr. Hatfield for his appreciative remarks. Dr. Hatfield wished to know whether the elastic range coincided with the stress at which the strain ceased to be proportional to the stress. That was so for the alternating stress tests, wherein there was proportionality up to a definite range of stress. But whether either extremity of this limiting elastic range coincided or not with the elastic limit determined statically (that is, by slowly increasing load) he could not say. He had made a torsion test up to the yield-point by slow loading, and the elastic limit, as estimated from the plotted curve of torque and twist-strain, appeared to be about 5·8 tons per square inch shear stress. But this estimation of the departure of a curve from a straight line was far from satisfactory, and it would perhaps be more accurate to say that the elastic limit lay between 5·5 and 6·0 tons per square inch of shear stress. It would have been observed that the limiting elastic range under alternate stressing was very closely $\pm$ 5·50 tons per square inch. He (Dr. Mason) did not claim to have established that the fatigue limit was higher than the elastic limit; but he did assert that the steel used would stand a very large number of repetitions of a range of stress intermediate between the elastic limit range (determined under alternations) and the range that caused fracture. In support of this assertion all the bending tests and also the torsion tests of solid specimens did not count as evidence; for it was obvious that in all these the stresses, calculated on the assumption of perfect elasticity, were larger than the real stress

(Dr. Mason.)

when the elastic limit was passed. He founded this assertion on the tests of the thin hollow test-specimens in torsion.

He had no absolute guarantee that the specimens used were not strained in machining; they were roughed out in a Liverpool works, but considerable work, including the final cuts upon them, was done in the engineering laboratory (Liverpool). The elastic limits, under alternations, were sufficiently uniform to substantiate the belief that such straining had not occurred.

He was glad to have Dr. Hatfield's suggestion that the presence of impurities would probably have an essential connexion with the appearance of the fatigue cracks in a direction parallel to the "grain." He had had only one torsion specimen in which the first-formed portion of the crack was perpendicular to the axis, that is, straight across the grain. This exception might be due to a circumferential tool mark left inside the specimen. Of course the planes most severely stressed in shear were planes through and perpendicular to the axis of specimen—planes which included all the fissures which could be distinguished as being the starting points of the cracks in the torsion specimens.

MEMOIRS.

Edward Christopher Blackstone was born in London on 14th August 1850. In 1868, after a course of instruction at King's College, London, he was apprenticed to Messrs. J. and H. Gwynne, Hammersmith Iron Works, London, for four years, and was afterwards employed by them as draughtsman. He was next engaged as assistant engineer in constructing the waterworks in Jersey, and he afterwards assisted in the office of Mr. W. H. Le Fevre, on railway and other work. He was for some time resident engineer with Messrs. Charles Nelson and Co., cement manufacturers, where he was employed in designing and superintending the erection of buildings and machinery. Mr. Blackstone was subsequently engaged as draughtsman in the Trinity House under Sir James N. Douglass. In 1877 he joined Mr. G. E. Jeffery of the Rutland Iron Works, Stamford, the firm being known as Jeffery and Blackstone. In 1882 Mr. Jeffery sold his interest in the business, and Mr. Blackstone was joined by Mr. Mills, a relative. The want of more room became pressing, and in 1886 a new works was erected off the Ryhall Road. The new workshops were designed and the construction superintended by Mr. Blackstone. Owing to the greatly increased demand for the products of the Rutland Iron Works, additional capital was needed, and in 1889 the business was converted into a limited liability company. From that date to the present time the concern has been known as Blackstone and Co., Ltd., and Mr. Blackstone has been managing director. He was the inventor of the first horse-power mechanical swath-turner in 1892, and since then his name has been associated with many improvements in these machines, which gained the first and second prizes at the Royal Agricultural Society's trials in 1907. In the making of hay this has been a great labour-saver, a pony and a boy doing the work of ten men. At this time he was also devoting his attention to what is known as a side hay-rake, and this machine won the first prize at the same trials. He brought out many improvements in

grass mowing-machines, horse-rakes, hay-tedders, corn-grinding mills, hay-loaders, and turnip-cutters. Probably his name will be best remembered by the Blackstone Oil Engines made under Blackstone and Carter's patents. He had been associated in the large number of 64 British patents. When he joined Mr. Jeffery in 1877 about 100 men were employed at their Works, and in 1914 this number had increased to fully 1,000. Mr. Blackstone was a Justice of the Peace for Stamford. He was a Member of the Royal Agricultural Society of England, and of most kindred societies in the kingdom. His death took place at Stamford on 6th October 1916, at the age of sixty-six. He was elected a Member of this Institution in 1898.

Sec. Lieut. **BERNARD EBENEZER BUMPUS, Northumberland Fusiliers,** was born in London on 9th February 1883. His education was received at local schools, and at the evening courses of the Finsbury Technical College from 1899 to 1901. He began his apprenticeship in January 1898 in the shops of Messrs. Johnson and Phillips, Ltd., and two years later went to the works of Siemens Brothers, Ltd. From 1902 to 1904 he worked in the submarine cable department of the India-rubber, Gutta Percha, and Telegraph Co. of Woolwich. On the completion of his apprenticeship he was engaged as engineering assistant in the London office of Messrs. Dick, Kerr and Co., Ltd., until 1908, when he was appointed mechanical and electrical engineer to the Bibiani Mines, Ltd. In the following year, on behalf of Messrs. Dick, Kerr and Co., he made a six months' tour to the West African mines to report on business extensions, and on his return to this country he was employed in the engineering department of the same firm. In 1914 he went to India to take up a position with the Bombay Electricity Supply and Tramways Co., and early in 1915 volunteered for military service. Subsequently he received a Commission as 2nd Lieutenant in the Northumberland Fusiliers, and took part in the fighting in France, where he lost his life on 3rd July 1916, in his thirty-fourth year. He was elected an Associate Member of this Institution in 1914.

Co. Sergeant-Major **ALLAN WILLIAM CORFE** was born at Liscard, Cheshire, on 23rd November 1891. His early education was received at local schools, after which he attended the Liscard School of Art and the Central Technical School, Liverpool, where he passed various Board of Education examinations. In June 1906 he was apprenticed to Messrs. T. Ollis and Co., of Liverpool, printing machine manufacturers, and in February 1908 he continued his apprenticeship at the ship-repairing works of Messrs. H. and C. Grayson, Birkenhead, where he remained two years, and at the shipbuilding works of Messrs. Cammell, Laird and Co., Birkenhead, subsequently spending six months in the drawing office. On the completion of his apprenticeship he became draughtsman at the works of Messrs. Mirrlees, Bickerton and Day, Stockport, and was in their employment on the outbreak of the War. Previously he had entered the Cheshire Territorial Regiment as a private, and successively passed through every stage in the non-commissioned ranks until he was made Company Sergeant-Major. He was recommended by his Commanding Officer for a Commission shortly before his death, which resulted from wounds received in France, on 26th November 1916, at the age of twenty-five. He became a Graduate of this Institution in 1912.

Lieut. **CHARLES LAMBERT DRUITT**, Seaforth Highlanders, was born at Christchurch, Hants, on 14th October 1888. After attending a preparatory school at Southbourne, he went to King's School, Canterbury, and on leaving there in 1907 he took the three years' course at the Central Technical College, South Kensington, obtaining the College Diploma. In 1910 he worked for one year in the locomotive shops of the Barry Railway Co., South Wales, and then was articled for three years to Mr. P. C. Tempest, the chief engineer of the South Eastern and Chatham Railway, and subsequently was engaged as assistant district engineer on that line. In 1915 he joined the 9th Seaforth Highlanders (Pioneers) as 2nd Lieutenant, and was engaged in the Battle of the Somme from its commencement. In August 1916 he was promoted to be full Lieutenant, and was mentioned in Sir Douglas Haig's dispatch

of 13th November. He was killed near Eaucourt l'Abbaye on 13th October 1916, at the age of twenty-eight. He became a Graduate of this Institution in 1912.

George Eaton-Shore was born at Crewe on 3rd February 1853. He commenced his engineering career with the London and North Western Railway Co. at Crewe Locomotive Works, the last piece of work he carried out for the Company being the designing of the new foundry under the supervision of the late Mr. F. W. Webb. In 1882 he was appointed Borough Surveyor of the town of Crewe, and this position he held until the time of his death. During his 34 years of office the population of the town increased from 24,000 to 45,000, and in consequence miles of streets and sewers were laid out under his supervision. Other work comprised the design and erection of the town's depot and fire-station, and the carrying out of the new sewage works which were constructed to deal with $1\frac{1}{4}$ million gallons of sewage per day. His death took place, after a long illness, on 6th November 1916, in his sixty-fourth year. He was elected a Member of this Institution in 1888.

Jesse Ellis was born at Cranbrook, Kent, on 14th April 1846. He received his engineering training of five years with the firm of Messrs. Aveling and Porter, Rochester, and, on leaving them at the age of twenty-three, started in business for himself. From 1871 he was the managing partner of the firm of Jesse Ellis and Co., engineers and boiler makers, Invicta Works, Maidstone, where he made a specialty of traction engines, and brought out a steam motor-wagon with special fire-tube boilers. This wagon he personally introduced into Egypt in 1902, and was complimented by the Sirdar, Sir Reginald Wingate, on its success. He was one of the founders of the Royal Automobile Club, and a Member of the Royal Agricultural Society, and of the Bath and West of England Show. He was also one of the founders of the National Traction Engine and Owners' Association, and a Vice-President of that body at the time of his death; for a great many years he did constant and most useful work in connexion with heavy road

haulage. For twenty-five years he was a member of the West Kent Yeomanry, and retired when holding the post of Farrier Major. His death took place, after a long illness, on 17th October 1916, at the age of seventy. He became an Associate Member of this Institution in 1898, and a Member in 1904.

Captain William Henry Jaques was born in Philadelphia, Pa., on 24th December 1848, being descended from an old Huguenot family, which early went to America. Having been educated at schools in New Jersey, he entered the United States Naval Academy in 1863 as midshipman, and graduated in June 1867, when he went immediately into active service. He was promoted Ensign in 1868, Master in 1870, and Lieutenant in 1871. During his active service in the Navy he served on board the "Constitution," "Marblehead," etc., and the monitors "Saugus" and "Manhattan." At various times he performed duties as Aide to the President, Secretary of the Navy, and Commandant of the New York Navy Yard. From 1870 to 1874 he was assistant in the United States Coast Survey, and from 1874–78 assisted the New York Board of Education. In 1881 he became Assistant Inspector of Ordnance; in 1883 Member and Secretary of the United States Gun Foundry Board; and in 1886 Secretary to the Senate Committee on Ordnance and Warships. Captain Jaques introduced into the United States the system of fluid compression and hydraulic forging of heavy masses of steel, and was the inventor of many improvements in the manufacture of heavy ordnance and armour, and was the leading exponent of the employment of nickel in steel. He was for some time associated with Captain John Ericsson in the development of submarine artillery.

In 1887 Captain Jaques resigned his commission in the United States Navy, and accepted a position with the Bethlehem Iron Co. (now the Bethlehem Steel Co.) to superintend the design, construction, and adaptation of machinery for the establishment there of ordnance and armour-plate works. He continued as ordnance engineer until 1894, when he retired, and then became associated with Mr. Horace See, engineer and naval architect, in

general engineering and consulting work, especially in connexion with ordnance material. At the beginning of 1895, at the request of the Governor of New Jersey, he began the organization of a Naval Reserve for that State, and shortly afterwards was commissioned Captain. This command he held until January 1898, when loss of health compelled him to resign. The later developments of the Bethlehem Steel Works were in a great measure due to Captain Jaques, who became the recognized independent authority in the United States on the manufacture of guns and armour.

Captain Jaques was presented with the Whitworth Scholarship medal for his metallurgical work, and was decorated by the Emperor of Japan with the Order of the Rising Sun for his services to Japan previous to the Chinese War. In 1897 he undertook the development of submarine torpedo-boats, and accepted the presidency of the Holland Submarine Boat Co.; and during this period the mechanical control of submersion was perfected. He was the author of numerous books and monographs on heavy ordnance, armour, torpedoes, solar radiation, &c. In the summer of 1916 he came to England to reside for a time with his family at Hadley Wood, near London, and was killed on the railway line near that station, by stepping out on the wrong side of the railway carriage, on 23rd November 1916, in his sixty-eighth year. He became a Member of this Institution in 1888, and was a Member of the Institution of Civil Engineers, the Iron and Steel Institute, an Associate of the Institution of Naval Architects, and was a Member of the leading American Scientific Societies.

William Walter Lupton was born at Leeds on 28th January 1844. He was educated at schools in Brighton and Worksop, and under a tutor at Worms in Germany; at the age of nineteen he entered the works of Messrs. Joshua Buckton and Co., Leeds, as a premium apprentice. On the completion of his time four years later, he continued with the firm as draughtsman and excelled in the speed and correctness of his designing. Afterwards he became works manager, and held this position for

twenty-five years with great success. On the conversion of the firm into a limited company he was appointed managing director, and carried on the practical works management for about twelve years; after which he relinquished active management in 1896, and became a director of the Company. He then removed from Roundhay, near Leeds, to Malton, where he was placed on the Commission of the Peace for the North Riding of Yorkshire. He was ex-president of the Malton Rifle Club, and raised a cadet corps. During the War he had shown a keen interest in the conditioning of Army remounts. His death took place at Malton on 4th November 1916, in his seventy-third year. He became a Member of this Institution in 1900.

Alexander Malker McLean was born in Liverpool on 17th March 1878. He received his scholastic education at the Liverpool Institute until 1894, after which he studied at the Liverpool Technical School until 1899. In March 1894 he began a five years' apprenticeship with the Cunard Steamship Co., and on its completion he remained with the Company for a short time, being employed on the shore staff of engineers. In December 1899 he was engaged by Messrs. R. P. Houston and Co., and served in various grades of marine engineer until 1902, and in January of the following year he was a candidate for an Extra First Class Engineer's Certificate, gaining the second place in the British Isles. He then became engineer surveyor to the Ocean Accident and Guarantee Corporation, Ltd., and subsequently was appointed to control their engineering business in Ireland. He held this post until 1910, when he accepted an invitation by the Gresham Insurance Co. to become chief engineer and manager of their engineering department. This position he held until his death, which took place at Onchan, Isle of Man, on 18th November 1916, in his thirty-ninth year. He was elected an Associate Member in 1904, and was transferred to Membership in 1914.

Sir Hiram Stevens Maxim was born at Sangersville, Maine, on 15th February 1840. His ancestors were Huguenots and came to

England at the revocation of the Edict of Nantes. Early in the eighteenth century they emigrated to Plymouth, Mass., and a hundred years later went to Maine. At the age of fourteen his father apprenticed him to a coach-builder, and during this period he was credited with having constructed the first tricycle built in America. Soon after this he worked at his uncle's engineering works at Fitchburg, Mass., and later was at a philosophical instrument maker's, also at a shipbuilder's. He was for a time with Mr. Oliver P. Drake, a maker of gas-machines at Boston, and while so engaged invented in 1865 a machine for making lighting gas by means of vaporized petroleum. He also perfected an automatic sprinkler, and made improvements in feed-water heaters, steam and vacuum pumps, engine governors, gas-motors, etc. In 1878 he became chief engineer of the first electric lighting company in the United States, which was founded by Mr. S. D. Schuyler. While so engaged he invented the process of flashing carbon filament lamps in a hydrocarbon vapour, and took out patents for dynamos, lamp carbons, secondary batteries, &c. In 1881 he went to Europe to represent his firm in Paris, and while there he was made a Chevalier of the Légion d'Honneur, on account of his invention of an electric pressure-regulator. It was at that time he became interested in machine-guns, and was introduced to Mr. Albert Vickers, who at once recognized the value of the invention, which surmounted the difficulty of cartridges jamming in the barrel. The Maxim Gun Co., with Mr. Vickers as chairman, was founded in 1884, and in 1888 amalgamated with the Nordenfeldt Co. Later, these were absorbed in the Vickers firm, which then became Vickers, Sons, and Maxim. The Maxim gun was adopted by the British Army in 1889, and by the Navy in 1892, and it has been supplied in large quantities to all the military powers of the world. When he ceased to be a director of the firm on his seventy-first birthday, in 1911, the title was again changed to Vickers Limited. It is reported that, on the suggestion of Lord Wolseley, he invented a smokeless powder, and evolved the Pom-Pom gun.

The question of aviation had interested Maxim from his youth, but he never achieved successful flight. Many experiments were

carried out during 1892–4 at Baldwin's Park, Kent, and the most novel features were the engines and boilers; but their weight, together with that of the feed-water and fuel, precluded their success. A Paper on this subject was read by him in 1894 at a Meeting of the British Association. The light motor, to which aeroplanes now owe their success, had not at that time been developed. One Paper he contributed to this Institution, namely on "The Maxim Automatic Machine-Gun" (Proceedings, 1885, page 167). Honours and decorations were accorded to him in most European countries, and he received the Order of Knighthood in 1901. He had a rare genius for invention, and, while discarding failures, took great pride in his successes, which ranged over a great variety of ideas. His death took place at Streatham on 24th November 1916, in his seventy-seventh year. He was elected a Member of this Institution in 1884.

John Pratchitt was born at Chester in 1833, and was educated at Chester College. After having served an apprenticeship with Messrs. Hick, Hargreaves and Co., of Bolton, he went as draughtsman to the firm of Pratchitt and Blaylock in 1860, whose works were then situated at Long Island Iron Works, Carlisle, but were subsequently removed to the Denton Iron Works, Carlisle. In 1866 he became managing partner of the firm, the title being altered to Pratchitt, Blaylock and Pratchitt, and in 1870 to Pratchitt Brothers. He was connected with the carrying out of several large schemes, both sewage and water, namely, at Tunbridge Wells, Dorking, Exmouth, Worthing, Southend-on-Sea, Reading, etc. He designed and carried out the pumping machinery for the late King Edward VII at Sandringham, and supervised the making of machinery for sewage plants at Rangoon and Henley. His activities were not confined to pumping machinery, as he travelled in Germany and Greece and made several engines and grinding mills for both countries, as well for this country, and when the Lancaster and Carlisle Railway was in construction his firm built many of the stations, etc. His death took place at Carlisle on 9th December 1916, at the age of eighty-three. He was elected a Member of this Institution in 1867.

Lance-Corporal **WILLIAM ARTHUR REYNOLDS**, Royal Warwickshire Regiment, was born in London on 24th November 1892. He was educated at schools in West Ham, and gained a scholarship at West Ham Technical Institute, graduating B.Sc. (Eng.), University of London, in 1913. When war broke out he was an apprentice at Messrs. Thornycroft's Shipbuilding Works at Southampton, and was a member of the 5th Hants Territorials. Early in 1916 he was transferred to the Royal Warwickshire Regiment, and went to France in May. On 28th October 1916 he was wounded by an aerial torpedo and died the following day, in his twenty-fourth year. Shortly before his death he was granted a Commission in the Royal Engineers, but unfortunately he did not live to hear of his success. He was elected a Graduate of this Institution in 1912.

Sec. Lieut. **ROBERT SERGIUS ROBERTSON**, 1st King's Own Scottish Borderers, was born at Ponce, Porto Rico, West Indies, on 4th September 1893. His primary education was received at Albert Road Academy, Pollokshields, Glasgow, and afterwards at Allan Glen's School. Leaving there he took the Diploma Course at the Royal Technical College, Glasgow, gaining the Associateship. During the last year of his studies, he was at the University of Glasgow and took his B.Sc. degree in Engineering. He received his practical engineering training partly with Messrs. Blackadder Bros., Falkirk, and Messrs. Gauldie and Gillespie, of Kinning Park, Glasgow. From the O.T.C. of his University he received his Commission in March 1915, being attached to the 1st King's Own Scottish Borderers, and was with his battalion in Gallipoli from the beginning of October 1915 until the evacuation of our forces. He was transferred with his unit to France, and was wounded at Beaumont Hamel on 8th July 1916, his death taking place at Rouen on 20th July, in his twenty-third year. He was elected a Graduate of this Institution in 1914.

Corporal **LIONEL EGERTON VYALL**, Canadian Division, was born at Chawant, Punjab, on 11th March 1889. After

attending a preparatory school he went to Cheltenham College for three years, and finally to the Central Technical College, South Kensington, where he completed his three years' course in 1909, obtaining the College Diploma and B.Sc. (Eng.) degree, University of London, with honours. He then underwent his practical training for three years as a pupil at the locomotive works of the Rhymney Railway Co., Caerphilly. In 1911 he was selected for an assistant engineer's appointment in the Public Works Department of India, but was unfortunately disqualified owing to defective eyesight, and then became an assistant inspector at Glasgow on Sir Alexander Rendel's staff, which he held for a year. In April 1913 he went to Canada, and worked for short periods in the Canadian Pacific Railway workshops, the Dominion Bridge Engineering Co., and other firms in Montreal. When the War broke out he enlisted as a dispatch rider in the 2nd Signal Company, 2nd Canadian Division, and underwent his training at Ottawa, and subsequently at Shorncliffe. In September 1915 he went with his Division to France, where he served for a year. On 3rd September 1916, whilst carrying dispatches between St. Omer and St. Martin, he met with a fatal motor-cycle accident at night. He was in his twenty-eighth year. He became a Graduate of this Institution in 1911.

James Walker was born at Cupar, Fife, on 21st March 1873. He was educated at Montrose Academy from 1879 to 1889 and studied mechanical and civil engineering for two sessions at Dundee and Glasgow Technical Colleges, gaining several scholarships, including a Whitworth Exhibition in 1895. His apprenticeship was commenced in 1889 with Messrs. Parker and Sons, Victoria Foundry, Dundee, and a year and a half later he went to Messrs. Gourlay Bros., engineers and shipbuilders, of Dundee. In December 1896 he joined the staff of Messrs. S. Pearson and Son, as assistant engineer on the Blackwall Tunnel Works, and remained with them on various contracts until 1904, in which year he was engaged by Messrs. J. G. White and Co., London, and was in charge of their tramway contract at Essendon, Melbourne, during 1905–6, and the Bombay Tramways contract in

1907–8. On the completion of the latter contract, he went to Japan with a party of engineers engaged in investigating a proposed hydro-electric scheme, after which he was chief engineer's assistant in their head office. In May 1914 he was engaged by Messrs. Lever Bros. as superintendent engineer in connexion with their palm-oil industry, and while on a visit to the Belgian Congo he became ill, and died on arriving at Falmouth, on 2nd December 1916, in his forty-fourth year. He became a Member of this Institution in 1909; and was also an Associate Member of the Institution of Civil Engineers.

THOMAS JOHN WATSON was born at Newcastle-on-Tyne on 23rd June 1856. He served his apprenticeship from 1871 to 1877 in the engine works of Sir W. G. Armstrong and Co., Newcastle-on-Tyne, and was then engaged as draughtsman until 1890 in the same works. In the latter year he became chief draughtsman in the works of Messrs. Cowans, Sheldon and Co., Carlisle, and remained with them until 1892, when he started for himself as consulting engineer in Newcastle-on-Tyne, and continued in practice until his death, which took place at Humshaugh, Northumberland, on 27th October 1916, at the age of sixty. He was elected a Member of this Institution in 1897.

Professor HARRY MORTON WAYNFORTH was born in London on 19th December 1867. He received his early education at the Haberdashers' Company's Schools in Shoreditch, remaining there until 1883, when he was awarded a scholarship at the City and Guilds of London Technical College, Finsbury. On completing his course in Mechanical Engineering at the College in 1886, he passed through the shops of Messrs. Bennett, Sons and Shears, Ltd., brewery engineers, of Kingsland Road, London, and from 1889 to 1891 was assistant to Professor Perry at Finsbury Technical College. In the latter year he took an appointment as demonstrator in the Engineering Department of Mason's College, Birmingham, where he stayed until 1896, when he became demonstrator of Mechanica Engineering in King's College, London. This post he held until

1902, when he was appointed Assistant Professor of Engineering by the Council of King's College. In addition, he acted as an examiner to the City and Guilds of London Technological Examinations, of the London County Council, and for the B.Sc. Engineering Degree of the University of London. In 1912 he was appointed Professor of Engineering at King's College. His death took place, after a long illness, on 5th November 1916, in his forty-ninth year. He became an Associate Member of this Institution in 1897, and a Member in 1907. He was also an Associate Member of the Institution of Civil Engineers.

The Institution of Mechanical Engineers.

PROCEEDINGS.

March 1917.

An Ordinary General Meeting was held at The Institution of Civil Engineers, London, on Friday, 16th March 1917, at Six o'clock p.m.; Michael Longridge, Esq., *President*, in the Chair.

The Minutes of the previous Meeting were read and confirmed.

The President announced that the following fifty-four Candidates had been duly elected:—

MEMBERS.

Bayliss, Arthur William,	Birmingham.
Carr, John Francis,	Glasgow.
Donaldson, David,	London.
Hallett, Arthur,	Dublin.
Harris, Richard Henry,	Manchester.
Hunter, James,	Leven, Fife.
Knowles, William,	Manchester.
Lee, Norman Arthur,	Newcastle-on-Tyne.
Lever, Harry Edgar,	Carlisle.
Scott, Alfred Strachan,	London.
Scott, Patrick Murray,	Richmond, Surrey.
Simpson, Edmund,	Stockport.

ASSOCIATE MEMBERS.

Beevers, Leslie,	Wakefield.
Blackburn, Robert,	Leeds.
Bredin, Edgar Craven,	Dublin.

Bryan, Charles James Prentice,	Grays.
Butler, Arthur,	London.
Cartland, Walter Cuthbert,	Howrah.
Clarke, Arthur Cecil, Lieut. R.E.,	Longmoor Camp.
Dallas, James McKenzie,	London.
Davison, William,	Nottingham.
Dixon, Harold Leslie,	Leatherhead.
Ellison, Harold Thomas,	Sheffield.
Green, Wilfred Harry,	Sheffield.
Hamilton, Harry,	Manchester.
Hanson, Arthur,	Rochdale.
Heron, George,	Singapore.
How, Stanley Murdoch, Sec. Lieut. R.E.,	B.E.F.
Imlay, James Cameron,	Bluff, N.Z.
Lacon, Bernard John,	London.
Lincoln, Herbert Harold,	Sheffield.
Linley, Percy,	London.
Lowe, Robert,	Samara, Russia.
Lowndes, Percy Norman,	Singapore.
Mellor, Wilfred,	Singapore.
Midgley, Percy,	Rochdale.
Morfey, Frederick Henry,	Ipswich.
Morgan, John Bulmer,	Horsham.
Pinto, Liguari Simon,	Darlington.
Platt, Wallace Te,	Singapore.
Pollard, Hugh Bertie Campbell, Capt. M.I.,	London.
Raisin, Charles,	Dartford.
Reith, John Charles Walsham,	London.
Rooney, Marcus Hugh,	Malacca.
Schutte, William Matthew,	Poona.
Stevenson, Cecil Young, Captain R.E.,	London.
Tuppen, Harold Richard, Captain A.S.C.,	B.E.F.
Tyrer, Oswald Bertram,	Nottingham.
Wade, Edwin Dominic,	Bloemfontein, O.R.C.
Wilkinson, George Robert,	S. Farnborough.

GRADUATES.

Bose, Naba Kumar,	Glasgow.
Edmondson, John,	Barrow-in-Furness.
Main, Alfred Charles,	Melksham.
Townend, Frank Sutcliffe,	Birmingham.

The President announced that the following two Transferences had been made by the Council:—

Associate Members to Members.

Wilson, Percy Hutchinson, . . .	Chesterfield.
Wright, Hugh, ...	Dudley.

The following Papers were read and jointly discussed:—

"The Heat Treatment of large Forgings"; by Sir William Beardmore, Bart., of Glasgow, *Member*.

"Heat Treatment of Steel Forgings"; by H. H. Ashdown, of Newcastle-on-Tyne.

The Meeting terminated at a Quarter to Eight o'clock. The attendance was 66 Members and 54 Visitors.

The Papers on "The Heat Treatment of Forgings" were further discussed in Birmingham, in the Lecture Hall of The University, Edmund Street, on Thursday, 22nd March 1917. Mr. A. A. Remington, *Member*, presided. The attendance was about 70 Members and Visitors.

THE HEAT TREATMENT OF LARGE FORGINGS.

By Sir WILLIAM BEARDMORE, Bart., of Glasgow, *Member*.

In the heat treatment of large forgings there are no metallurgical principles involved which do not apply with equal force to the heat treatment of small forgings, such differences as exist being entirely due to the limitations which large forgings impose on the practical conditions under which the heat treatment is carried out. For every class of forging it is desired that the material from which it is made should give the mechanical tests required by a suitable choice of composition, but in addition it is most necessary that the material should be in such a physical condition that brittleness and the chance of sudden failure should be reduced to an absolute minimum.

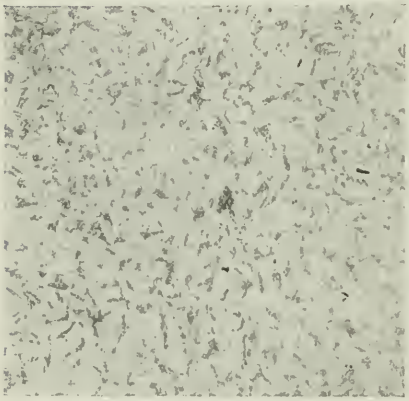
In carbon-steel forgings produced to meet a definite mechanical test specification, this is equivalent to saying that the steel must possess the least crystalline growth or the smallest grain size, and the object of all heat treatment is to confer this condition on the forging before it leaves the steel-works. It is well known that, at any chosen temperature, the time the material is kept at its heat and the time taken to cool down to normal temperature again have an all-important influence on the grain size, and it is for this reason that in large forgings all the difficulties of heat treatment are magnified; and, in the Author's opinion, a limit is ultimately

reached in the size of the forging beyond which a plain carbon-steel cannot be used with safety, and the use of an alloy steel becomes imperative.

The slowness with which heavy masses of steel cool down results in large crystal grains at the centre of the mass, and, to avoid this, recourse has to be made to oil-quenching to hasten the cooling down, and so to diminish the size of the grain, while a subsequent reheating is generally necessary to remove the hardness introduced by the quenching operation. With small forgings a simple annealing will put the material into the condition which will give

Oil-hardened 18 *inch Shaft.* × 100 *diameters.*

Fig. 1.—*Outside skin.* Fig. 2.—*Inside.*



satisfactory and safe results, but for large forgings some form of heat treatment is very necessary to get the best results.

It is a fact which is not sufficiently realized by engineers that in plain carbon-steel the effect of oil-quenching is not uniform over the cross-section, but diminishes the farther one goes from the outside surface. With very large forgings, therefore, a stage is reached at which, owing to the size of the forging, the quenching effect at the centre is so small that it is insufficient to confer any benefit on the material, and it becomes impossible to guarantee the uniform results which are wanted. That this effect is a real

one can be seen from the accompanying photomicrographs of a large oil-treated shaft and from the tests taken. This shaft is 18 inches diameter, and of the following percentage composition :—

C.	Mn.	Si.	S.	P.
0·18	0·65	0·10	0·048	0·037

It was heated in 820° C. (1,505° F.), kept two hours and cooled in oil. Tests were taken from the outside skin and centre, and gave results as shown in the following Table :—

—	Elastic Limit.	Ultimate Stress.	Elongation on 2 inches.	Reduction of Area.
	Tons per sq. in.	Tons per sq. in.	Per cent.	Per cent.
Outside .	17·5	33·1	27·0	53·1
Centre .	15·0	29·0	31·0	46·0

The structures are given in photomicrographs, Figs. 1 and 2, being the outside and inside respectively. The tests taken from the centre of the shaft have a lower tonnage than those from the outside, due to the fact that the quenching is not so effective. Photomicrograph, Fig. 2, confirms this. With a higher carbon steel it can be understood that the difference will be greater.

The effect of work is very important, and there is an intimate relation under practical conditions between the grain condition of a forging and the amount of work which has been put on it during forging. For the very best results, the ratio of the cross-section of the ingot and the largest cross-section of the forging should have a minimum value which it is advisable should not be less than three. Preferably it should be as great as possible, but it cannot always be adhered to, since for large sizes it is limited by the maximum size of ingot with which the presses can deal and sometimes by the shape of the forging itself. For an ingot 83 inches in diameter the maximum size of the forging should not exceed 48 inches for the best practice, and from a steel-maker's point of view, until the demand warrants the outlay of plant for dealing with still larger

ingots, larger forgings than this should be built up rather than manufactured in one piece. In this connexion the cause of commercial efficiency would be better served if engineers, in bringing out new designs in heavy steel construction, would consult the metallurgist and the steel manufacturer as to the best method of using their combined knowledge in the design and production of special requirements.

Though not strictly a forging, the following example illustrates very well what has been said above regarding the heat treatment of heavy forgings. The order specified steel disks 55 inches diameter and 11 inches thick with a mechanical test of—

Yield-Point.	Ultimate Stress.	Elongation (on ratio $\frac{\text{length}}{\text{diameter}} = 10$).
Tons per sq. inch.	Tons per sq. inch.	Per cent.
23	45	20

They were required to revolve at a high speed, and for magnetic reasons it was necessary to use a carbon-steel. The ingots were forged and then rolled to size from material of the following composition :—

C.	Si.	Mn.	S.	P.
0·53/0·54	0·18	0·50	0·029	0·040

and were given a heat treatment after this operation, which consisted of—

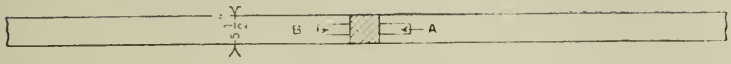
(1) Heating to 780° C. (1,436° F.) for 2 hours and cooling in oil

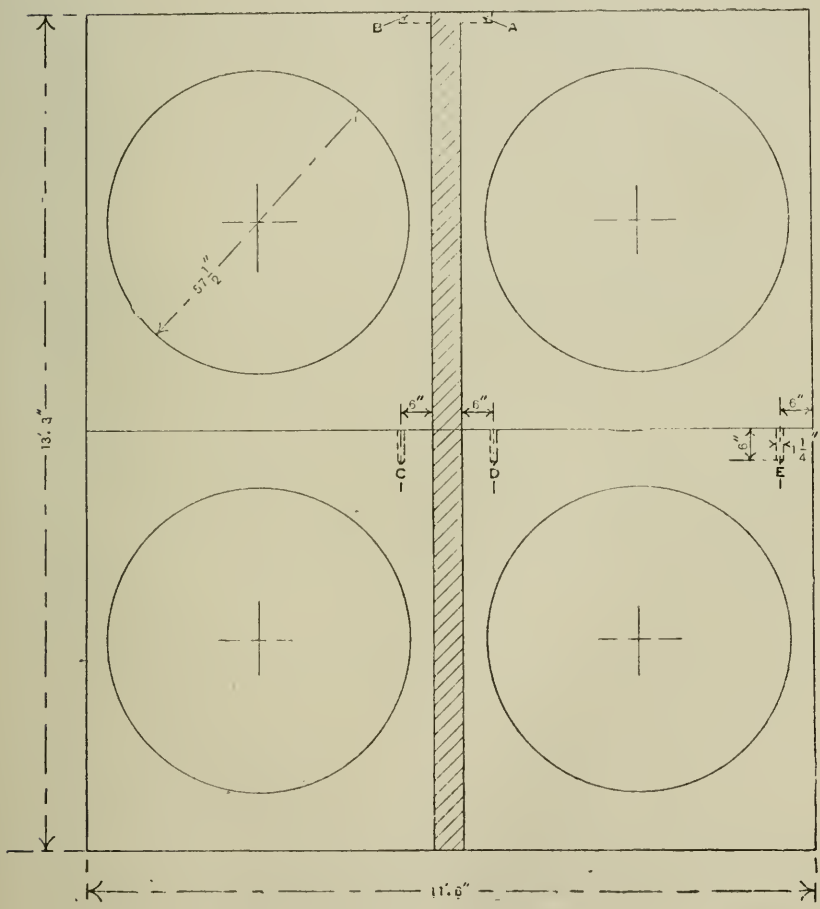
(2) Heating to 550° C. (1,022° F.) for 4 hours and cooling in air

Tests were taken from the centre of the slab (midway between the two surfaces), and they gave the following results :—

—	Yield-Point.	Ultimate Stress.	Elongation (on ratio $\frac{\text{length}}{\text{diameter}} = 10$).	Reduction of Area.
	Tons per sq. inch.	Tons per sq. inch.	Per cent.	Per cent.
Cross test .	23·6	42·3	14·0	39·3

FIG. 3.—*Forged and Rolled Slab used for Tests.*





This was the best that could be obtained, and was below the specified tests. There were two reasons for this; the carbon was too low and the thickness of the slabs was too great to allow the oil-quenching taking sufficient effect to attain the high tonnage required. Another ingot was therefore cast, from a higher carbon material giving—

C.	Si.	Mn.	S.	P.
0·62/0·63	0·25	0·70	0·023	0·024

and it was forged and rolled to 5½ inches thick to the size given in Fig. 3. In this way it was sought to avoid any unsoundness that might exist down the centre line. At the same time the test specification was amended so that the elongation required was 22 per cent. on 2 inches instead of 20 on a ratio $\frac{\text{length}}{\text{diameter}} = 10$.

The plate was heat-treated as follows:—

(1) Heated to 900° C. (1,652° F.), kept 1 hour and cooled in oil.

(2) Heated to 760° C. (1,400° F.), kept 1 hour and cooled in oil.

(3) Heated to 640° C. (1,184° F.), kept 8 hours and cooled in air,

and the following tests refer to the corresponding letter in Fig. 3.

Fig. 3.	Yield-Point.	Ultimate Stress.	Elongation on 2 in.	Reduction of Area.	Fracture.
	Tons per sq. in.	Tons per sq. in.	Per cent.	Per cent.	
A	28·1	46·2	28·0	52·4	Silky fibrous.
B	27·9	46·0	27·0	44·4	10% fibrous. 90% fine gran.
C	27·2	46·6	25·0	47·2	40% fibrous. 60% fine gran.
D	26·4	46·0	26·0	42·0	30% fibrous. 70% fine gran.
E	26·8	46·2	27·0	44·4	50% fibrous. 50% fine gran.

These results are satisfactory according to the test specification, and show the improvement effected by slight changes in the method

of manufacture. The improvement due to rolling to a smaller thickness alone was also seen when first material containing 0·53 to 0·54 per cent. carbon was rolled down from 11 inches to 5½ inches, and then retreated in the same manner, for it gave the following tests :—

—	Yield-Point.	Ultimate Stress.	Elongation (ratio length 10).	Remarks.
	Tons per sq. in.	Tons per sq. in.	Per cent.	In. thick.
Length . .	26·3	44·9	15·8	5½
Cross . .	25·7	44·2	16·5	5½
Cross . .	23·6	42·3	14·0	11

This result could have been still further improved by decreasing the thickness to 3 inches, but it shows how important the effect of work is on carbon-steels, and how it may make just that difference between success and failure.

For high-tensile material—that is, material over 40 tons strength—where the factor of safety is limited owing to the special conditions under which the material has to be employed, it will generally be safer to use an alloy steel, since for the same tonnage a very much tougher material can be developed. But for material whose tensile strength is below this figure there is not the same necessity, and excellent results can be obtained from oil-treated low-carbon steel.

Fig. 4 (page 222) indicates the outline of a large rotor-spindle which was made to the following test specification :—

Yield-Point	20 tons per sq. inch.
Ultimate Stress	36 ,, ,, ,,
Elongation on 2 inches . .	24 per cent.

and it will be seen from the drawing that the largest diameter was 3 feet 8¼ inches, and the minimum diameter only 11⅛ inches, so that if such an ingot were used as would give the requisite amount of work for the largest section, the amount of time and money spent in forging down this ingot to 11 inches would be out of all

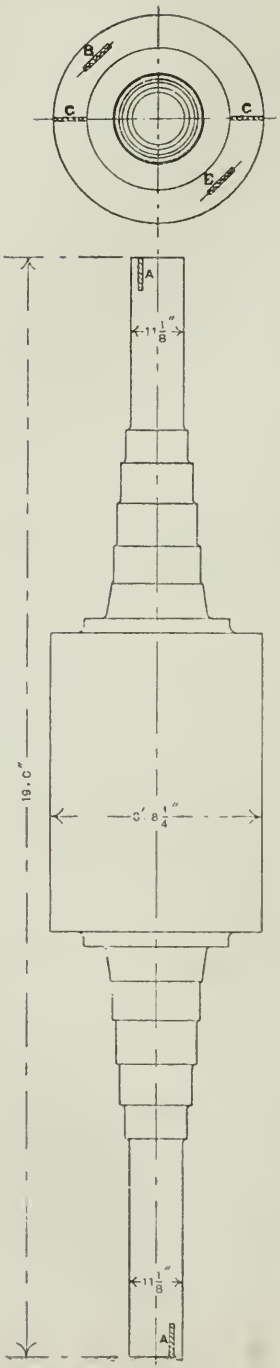


FIG. 4.—*Large Rotor-spindle.*

proportion; and so in this case, as in many other cases, the best metallurgical conditions are incompatible with the best commercial conditions, and a compromise has to be effected. An ingot 60 inches in diameter was used of the following composition:—

C.	Mn.	Si.	S.	P.
0·38	0·89	0·21	0·029	0·044

In the heat treatment of this job, and of jobs having similar shapes, when the forging is put into the furnace the time taken to heat up the heavy centre section is much longer than the time taken to heat up the end sections. For instance, the end-pieces of 11 inches diameter could be heated up in about 4 hours, while the centre would take about 20 hours to reach the same heat, so that before the forging would be uniform the end-pieces would have been at their heat for 16 hours, and would have grown such a crystalline grain in consequence that any good effect of the oil treatment would have been annulled. To avoid this, the ends were covered with asbestos sheets, leaving only the centre exposed when the forging was charged into the furnace, and this sheeting was removed after 15½ hours in the furnace, so that the forging attained its heat uniformly as a whole, and the crystal grain in consequence was uniform throughout.

The test results obtained were very good, and are given below. A refers to length tests taken one from each end of the forging, B to tests taken circumferentially from a ring taken from the centre part after treatment, and C to tests taken radially from the same ring. In both B and C tests two tests were taken from opposite diameters :—

Fig. 4.	Yield-Point.	Ultimate Stress.	Elongation (on 2 inches).
	Tons per sq. inch.	Tons per sq. inch.	Per cent.
A	22·6	38·8	27
	22·8	39·2	27
B	22·8	39·2	28
	22·4	38·8	28
C	22·4	38·8	30
	22·4	35·8	24

The treatment which was given to this rotor was :—

(1) Heated to 800° C. (1,472° F.) for 2 hours and cooled in oil. The centre part attained its heat in 19 hours 30 minutes, and the ends were uncovered after 15 hours 30 minutes, so that they attained their heat simultaneously with the centre part.

(2) Heated to 550° C. (1,022° F.) in 11 hours 30 minutes, and kept at heat for two hours.

A very troublesome feature of large forgings which have been heat-treated is the distortion which takes place, due to internal stresses probably set up by the operation of quenching. On machining the forging, and especially on removing the outside surface, these stresses are partly relieved, and the forging distorts and takes up a new shape under the influence of the forces still left in the material, so that it loses straightness and generally adds very largely to the time occupied in machining. It is advisable on this account to rough-machine the forging after treatment, and then to re-anneal it before the final treatment is done. Allowance for this re-annealing can be made on the first testing, and where

the final machining is intricate and accurate it will be found to well repay the cost, since trouble in the machine shops due to distortion will be reduced to a minimum.

The Paper is illustrated by 4 Figs. in the letterpress.

HEAT TREATMENT OF STEEL FORGINGS.

By H. H. ASHDOWN, of Newcastle-on-Tyne.

So much has already been said and written concerning the heat treatment of steels, and one finds in all works, where any form of treatment is practised, so many authorities on this subject, that the lay mind would think that perfection in every possible way had been achieved. Further, with the perfect system of pyrometric control now installed in most of our steel works, the impression gained is the impossibility of failures to occur due to incorrect treatment.

The heat treatment of steel may cover a very wide field, but the heat treatment of steel forgings suggests at once the impression of handling materials both in quantity and in the mass. To give a broad sense of comparison, one might compare the treatment of a piece of 1-inch round steel with a forging weighing some 60 tons, or a furnace full of forgings in the aggregate weighing 200 tons—this being on a parallel with the vastly different conditions ruling when casting ingots of a few cwts. in weight and those weighing 100 tons. From experimental evidence and casual observation it is quite evident that there is still much wanting in this direction, in spite of our greatly advanced knowledge and high attainments in research on this subject of recent years.

[The I.Mech.E.]

The Author has been closely associated with the investigation, manufacture, and treatment of steels in all their phases for the past twenty-five years, and for the greater part of that period has dealt with them in a commercial sense. His proposal therefore is to lay this matter before the Institution in as practical a way as possible, dealing with the subject from a commercial or works point of view rather than approaching it as a subject of research.

Having thus mapped out the course of the Paper, the Author has decided to exclude, as far as possible, technical terms relative to structural conditions, and in the subsequent discussion he proposes to evade such technical references, as he finds the nomenclature of metallography, generally speaking, more confusing than helpful to the practical man.

In order to introduce and prove the value of metallography in the works, the Author has found it expedient to submit photomicrographs together with some form of practical illustration, an example of which is shown in Fig. 5, Plate 4, which shows at sight the fundamental principles underlying a moderately careful treatment, and then to compare their relative comparative strengths with that of bricks and mortar and concrete. It will of course be appreciated that these photomicrographs are strictly comparative, each being of 100 magnifications. At the outset he ventures to suggest that the greater percentage of failures occurring is not so much due to the lack of necessary scientific knowledge, but from the want of knowing how to apply that knowledge in works practice, and also in a large measure from the lack of observation generally, when forgings are subjected to heat treatment.

The Author would here introduce a rough outline sketch of a furnace about 8 feet by 10 feet by 100 feet long, Fig. 6, containing miscellaneous forgings such as are now comparatively common in big commercial steel works. These furnaces are periodically charged up with forgings of all descriptions, say, for example, heavy gun forgings, marine shafting, turbine shafting, and the like, possibly amounting to 150 tons in the aggregate. In the first place they are heated to a predetermined temperature. Annealing temperatures vary greatly, from anything between 650° C,

(1,200° F.) and 927° C. (1,700° F.), and the results obtained from the forging naturally vary accordingly. These temperatures are questioned at times, and one is told that there is little need to trouble, as the forgings as a rule pass test. That may be so, but unfortunately one hears from time to time of forgings, machine parts of all descriptions, breaking down prematurely, due to fatigue. Fatigue compares very favourably with a medical practitioner's "influenza"; it temporarily covers a very wide field of inherent troubles. The cause of the so-called fatigue could often be located at the manufacturer's works before such forgings were put into service.

In Figs. 7 and 8, Plate 5, are seen various types of structures from similar steels in the so-called annealed condition. The Author would prefer not to suggest anything that might be embraced in already stringent specifications, but this condition of things is doubtless a matter that must receive closer attention in the immediate future. The temperature of 650° C. (1,200° F.) is, as all know, well under the change-point of steels in general, and unless that critical temperature is exceeded comparatively little change is effected in the structural condition. A forging which has been finished at a high temperature, or parts which have been raised to a forging heat and have received little or no subsequent work, on cooling retain a very coarse structure.

To make this point more clear, the Author submits as an example two operations in the forging of a marine shaft, Fig. 9, Plate 6. By withdrawing the already cogged billet A from the furnace, the amount of work shown in B is performed. Portion B_1 is finished, but in order to heat up portion B_2 for the subsequent forging operation, portion of B_1 must also be included in the furnace, and this naturally becomes subjected to a prolonged forging heat, subsequently receiving little or no work upon it. The structural condition of this portion of such a shaft may not be unlike that of an unannealed steel casting, an example of the structure being shown in Fig. 10, Plate 6. A bad case of this description came before the Author's notice some little time ago. The forging in question was a double intermediate shaft which actually cracked in the lathe

during the turning operation; and, within the Author's knowledge, this is by no means an isolated case. The shaft in question was cut through this defective area, and tests taken in the immediate neighbourhood gave the results shown in the illustration.

The shaft was then subjected to a prescribed heat treatment, and the results of the test, together with the microstructure, are given side by side for comparison, Fig. 12, Plate 6. This regrettable feature applies not only to large forgings, but equally so the whole way down the scale, even to drop stampings. It is not his intention to name any particular forgings of which examples are given, nor their source of manufacture; suffice it to be said that it has been the privilege of the Author to visit many large steel works both at home and abroad.

By way of an example of a comparatively small forging, a special type of heavy bolt was made by heating to forging temperature for their full length pieces of round bars and "upsetting" portion to form the head as shown in Fig. 11, Plate 6. These forgings were subsequently normalized* (this in the Author's opinion is a very vague term) at 677° C. (1,250° F.); but when such bolts were subjected to a shock test, it was by no means uncommon for the head to shell off. The reason for this will be readily understood when one examines what had taken place in the forging operation; the only portion which had received any work was the material forming the head, and where this adjoined the body there were two entirely different structures.

Although Fig. 13, Plate 7, was not taken from this particular type of forging, it will show the great contrast in adjoining structures when forgings are finished at high temperatures and are not correctly subsequently heat-treated. Now after these forgings received a proper heat treatment, a uniformity in structure was obtained together with the disappearance of any further shelling of the head when subjected to the same repeated shock stresses.

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* The terms "normalizing" and "annealing" are well defined in the British Standard Specifications for Wrought Steels for Automobiles, published May 1916.

A misconception more or less common in heat treatment of steels is that full advantage can be taken of the initial forging heat, and that forgings or stampings finished at a high temperature will, on cooling, regain a fine structural condition. This is entirely wrong, and if it is desired to obtain the best normal structural condition, the heat in such forgings must be allowed first to fall below its recalescence temperature and then reheated somewhat above its calescence temperature and preferably cooled in air.

With regard to high-temperature annealing and high-temperature quenching for hardening, it is generally known that as soon as one has reasonably passed the calescence temperature of the particular steel under observation, a refining in the structure will result therefrom, and also that, practically speaking, the maximum hardness of the steel can be effected by quenching from this temperature. It is also known that after exceeding reasonable limits of this temperature, in conjunction with the time factor, the coarse structure redevelops, and develops more rapidly the higher the temperature. A most important point often overlooked is the comparatively long time taken to raise forgings of appreciable size or in quantity to these elevated temperatures, and it is during this unnecessary period, which is costly in both fuel and labour, that the forgings are caused to deteriorate rapidly. Some of the big industrial institutions are favourable to high-temperature quenching, and in the discussion it would be interesting to learn their reasons for this. The Author has never found it necessary to exceed, either for annealing or quenching, when treating forgings in the mass, a temperature of 843° C. (1,550° F.), and has had no reason to regret the results obtained.

Reference has already been made to the lack of observation during the process of annealing. Let us return once again to the annealing furnace, and give it a few moments' attention whilst the heat is being gradually taken up by the forgings. What does one notice? Naturally, the smaller or projecting sections become heated long before the larger or depressed portions. Assuming now, for the sake of argument, that the predetermined temperature is to be 870° C. (1,600° F.), it is easy to understand that if in a

short time the small sections have attained this heat, it will be a considerable period before the larger sections acquire this same temperature, and whilst the process of uniformity is being obtained the smaller sections have been most unduly heated. Such points as these are often the fruitful cause of many failures, and they, unfortunately, are points which are frequently overlooked, and it is here where a good furnaceman becomes invaluable. The Author has found it an exceedingly good plan first to get a good initial soaking heat through the forgings, just under the "change-point," say about 704° C. (1,300° F.), where, if at all, little harm is done by a long heating, and then gradually to raise the whole of the forgings to the predetermined temperature.

Pyrometers.—Pyrometers are of great value and service in indicating temperatures, but in inexperienced or careless hands they are more harmful than helpful, and so far as recorders are concerned, the Author would place little value on them without knowing that careful observation had also been kept on the furnace. The record simply indicates the temperature of the area in which the pyrometer is located, Fig. 6, Plate 4, and Figs. 14, 15, 16, Plates 7 and 8, and a great diversity of temperature may often be seen in large furnaces within a few feet of this position. In the Author's opinion, the greatest value of a recorder is the moral effect it has on the furnacemen. It certainly causes them to be more attentive than they otherwise might be to their fire control.

The Author gives one instance relative to the deception of pyrometers. Let them imagine the pyrometer being in contact with the shaft end of the forging, Fig. 16, Plate 8; one might then record the desired temperature for many hours, and yet the body of the forging could still be several hundred degrees below that indicated. Indeed, the Author has seen comparatively small engine parts, of which, within a length of three feet, after heating and quenching, one end has been in its softest possible condition whilst the other end has been practically unfileable. When it is fully realized that pyrometers simply indicate a local temperature, and that uniformity of temperature is entirely dependent on careful

observation, many of the so-called mysteries, together with their consequent troubles, will disappear.

Although only one form of pyrometer has been illustrated, the Féry radiating pyrometer, Siemens water pyrometer, and Brearley sentinel pyrometer, Fig. 17, Plate 8, each has its own sphere of usefulness. In the latter case, for instance, the sentinels can be placed in positions quite inaccessible to other forms of pyrometers and again can be laid in rotating bodies, their complete fusion denoting their prescribed temperatures.

Sand Annealing.—When it is generally realized that forgings, after their structures have once been refined by a correct heat treatment and are quickly cooled, give better mechanical and physical tests generally, the fallacy of sand annealing will soon disappear. These remarks apply equally so to those which have been quenched and have received a subsequent light annealing or tempering to conform to specified tests. It is sometimes an advantage to bury, in sand, forgings made from steels akin to those exhibiting air-hardening properties, with a view to rendering them soft for machining; but if such forgings after machining are to be assembled in the machine in this annealed state, it is then better to cool quickly and reheat slightly below the change-point.

If it is intended to anneal forgings in sand with the object of softening for machining, they should be fully covered before they have fallen below the critical temperature, and for comparatively small forgings hot sand should be employed. For the want of a little knowledge on this subject much apparent slackness is practised, and this in a large measure accounts often for much of the so-called hard and soft spots which cause endless trouble and expense in the machine shops. The steel-maker of course usually has all such troubles reflected back upon him, often being informed that his steel is badly made and wanting in uniformity.

Oil Quenching.—Attention has already been called to the want of uniformity in heating for annealing, and this applies equally to the case of forgings being heated for quenching, in order both

to improve their structural condition and also to effect a certain standard of hardness. It might appear somewhat elementary to embrace such remarks as these, but when one finds that such important essentials are commonly overlooked it is well to make it quite clear that all forgings, whatever their size, must be quenched only when uniformly heated above the change-point of the steel.

Excessive heating of forgings before quenching is not only productive of a bad structural condition, but also renders the cooling medium unnecessarily hot, and thus effects a comparatively slow cooling of the forging through its critical range, allowing partial precipitation of the ferrite to take place. It will be generally admitted that the quicker a forging can be cooled until the critical range has been passed the better will be its structural condition, and consequently its physical properties will be of a correspondingly higher order. This, in the Author's opinion, is a matter deserving of closer attention than it would appear to have received in the past.

Quenching Medium.—This point leads us up to the liquid used as a quenching medium. The Author trusts he will not be exceeding the privilege conferred on him if he mentions the subject of gun forgings. Of recent years marked developments have been seen in the size and weight of these forgings, until they have assumed ponderous masses and require for their quenching tanks of oil from 8 to 10 feet in diameter and approaching 100 feet in depth. If further developments are to take place, the quenching of these colossal masses will have to be seriously considered if we are to obtain the best results, and not only compete with, but lead, competitive nations.

Those who have experimented by water-quenching materials in the mass, know that greatly superior results can be obtained, and also that the structural condition is all that can be desired. The Author believes he is correct in stating that this form of quenching, if not in use in this country, is in operation in foreign countries, and of the forgings so treated which have come before him, both for microscopical examination and general testing, to say the least, the

results have been highly commendable. He is disposed to think that steel manufacturers would welcome the opportunity of the choice of quenching mediums both from a point of view of producing a better article, and also by dispensing with a costly fluid, and one which is attendant with great risks when large masses of steel are being hardened. It is possible that slight modifications in the analysis of forgings so treated may have to be made, and at the same time it must also be remembered that quenching mediums which are highly suited for treating large and heavy forgings are not necessarily the best for light forgings or those of thin section; but such matters as these may be safely left to the discretion of our experienced manufacturers.

Specifications.—Many specifications covering the composition and treatment of materials leave much to be desired, and suggest anything but progress. They bind the manufacturer in every possible direction, and thus restrict him in the production of an article much superior to that to which he is now limited. The Author might cite, as an example, specifications covering certain high-class forgings. They are specified to give after heat-treatment, say, 30 to 35 tons tensile with a combined elongation and reduction of area of, say, 80. If a manufacturer produces a better article giving 37 tons together with a sum of 90 representing the combined elongation and reduction, this forging would be rejected, and would have to be re-annealed to conform to the maximum stress. Suffice it to say this forging after the re-treatment, although conforming to specification, would be much inferior than when in its original heat-treated condition.

The Author hopes that engineers, as the steel manufacturers' customers, will correct him if his interpretation of specifications is incorrect. He looks upon specifications as a working basis, drawn up between the engineer and the steel manufacturer when entering into contracts, to be used as a guidance for both parties and with reasonable discretion by the inspecting engineer. If specifications were so framed that they were to be enforced to the letter, one could see little use for the intelligent engineer as an inspector.

Research.—As research investigation should be of great value and assistance to the steel manufacturer in guiding him out of his occasional troubles, the Author feels that the Paper would be greatly wanting in a most essential detail if reference were not made to this very important subject. In the Author's opinion the great value of research work is often lost to the works owing to its want of application. The research chemist may be a highly qualified man, and his conclusions as to the cause of failure within reasonable limits of being correct, but generally speaking he is not in a position to apply his reasoning into works practice, and his reports being often of a highly technical character are to some extent beyond the technical range of our good practical men. It would therefore appear that a gap yet exists between the works and the laboratory, and requires to be bridged across in such a manner that the full value of research investigation can be infused into works practice.

It is often stated that the fine structural conditions and excellent tests obtained by treating pieces in the laboratory cannot be obtained when treating masses in the works. This idea is totally wrong; by reasonably careful control of furnace conditions, and where quenching is necessary, regulating these conditions also, equally good results can be obtained. To substantiate this statement, in Fig. 18, Plate 8, is shown the structural condition of a forging weighing about 30 tons, which had been subjected to a prolonged heating before quenching. This was subsequently given a refining treatment, requenched and tempered, with the result shown in the adjoining photograph. This is by no means an exceptional case, and he questions if a much finer structure can be produced in the laboratory when dealing with a 1-inch diameter bar.

Ghosts.—Ghosts in steel forgings, as is generally known, are caused by the presence of non-metallic enclosures; and whatever form of heat treatment these forgings may subsequently receive, it is practically impossible to remove these ghost-marked areas. It is, however, within the Author's experience, possible to reduce the size of the ghost-marks, but the treatment necessary is both

drastic and costly, and possibly the remedy may prove little better than the evil. This is a subject for which there is yet much room for investigation; and although in some cases their presence need not be looked upon with any degree of alarm, there is no question that a steel free from ghosts is a much superior article. In any case prevention is always better than cure, and it is quite possible for the steel-maker to cast ingots of 100 tons in weight with the major portion entirely free from ghosts.

The Author has endeavoured to indicate briefly the principles underlying successful heat treatment on a commercial scale, and although he has in a small degree discounted the value of science in the works, no one is more anxious than himself to see it take a prominent place. This end, however, can only be attained by creating in those who have the handling of this class of work a personal interest, and this he is sure will be best achieved by occasional practical demonstrations.

The great value of correct and careful heat-treatment does not yet appear to be appreciated by many whom it should seriously concern. When it is generally realized that forgings made from the best steel produced can be hopelessly spoiled, due either to insufficient heating or by overheating, and that forgings such as are now under review may be worth many hundreds of pounds, and again from this same cause premature fracture may take place with a consequent great loss of life, this subject may be given closer attention and considered more as a commercial, scientific art rather than a crude form of craft.

In conclusion, the Author wishes to express his thanks to Mr. F. B. Trevelyan and the Directors of Sir W. C. Armstrong, Whitworth and Co., Ltd., in whose service the Author is now engaged, for allowing him to present this Paper to the Institution.

The Paper is illustrated by Plates 4 to 8.

Discussion in London.

The PRESIDENT, in moving a hearty vote of thanks to the Authors of the Papers, said that Mr. Ashdown was somewhat in the position of a scientific witness, and he rather regretted that he was not able to adminster to him the oath which was usually administered to such gentlemen, namely, that he should tell the truth, the whole truth, and nothing but the truth. He did not at all accuse Mr. Ashdown of not telling the truth, or saying anything that was not the truth, but he was bound to say there was a good deal more to be told than had yet been told. Fortunately there were experts present at the Meeting, who would no doubt act as cross-examiners and try to elicit further information. The pictures that had been thrown on the screen were very pretty to look at, but without any information as to the carbon content of the steel, the temperatures at which the transformations took place, or the etching material, they conveyed little information. Of course, it could be seen generally that one steel was overheated, and the other was what was called "normalized," but exactly what the constituents were he did not know. Whether they were cementite or pearlite, or some of the other things that few of the members knew very much about, he could not say. He hoped that the discussion would disclose the temperatures at which heat treatment ought to take place, with reference to the carbon content of the steel.

The resolution of thanks was carried by acclamation.

Sir ROBERT A. HADFIELD, F.R.S. (Vice-President), said he had read with much interest the two Papers by the President of the Iron and Steel Institute, Sir William Beardmore, on "Heat Treatment of Large Forgings," and Mr. H. H. Ashdown on "Heat Treatment of Steel Forgings." Coming from such authorities, the Papers were entitled to more than ordinary respect; statements put forward representing Dalmuir and Elswick would naturally cover some of the best practice of the day in this line of work. He thought the

members were fortunate in having obtained the Papers, especially at a time when both the Authors were so busily engaged. It was not long since he had the pleasure of going through the Elswick Works and seeing one of the finest forgings the world had ever produced. He wished he was at liberty to give the members some information with regard to it, but he could only say that as an Englishman he felt very proud to know that this country had turned out so satisfactorily, not in theory, but practically, what was probably the biggest product in that particular line of work that the world had ever achieved.

His own experience had not been in small numbers of large forgings, but in large numbers of small forgings. At the present time it would not be advisable to say how many tens of thousands were being turned out by his firm per week, but it was quite enough to make an appreciable effect upon the enemy's resistance and defences on that far-flung front "somewhere in France." With regard to those forgings, he was glad to say that in both small and large—the latter being very large—he was not aware that there had ever been a single case of these or other British-made steel and forgings having failed in firing through faulty material. Speaking of his own firm's experience in making those forgings, they were exceedingly careful to pay special attention to temperatures, whether of the re-heating furnaces or of the subsequent operations, by means of optical and other pyrometers. There was no question whatever that the old-fashioned way of taking an ingot or forging and heating it up, as the forgeman would say, to a "certain colour," indicating what the forgeman thought was correct, was passing away. In other words, it was absolutely necessary to take temperatures at every stage, not only of the furnaces, but, in the case of heavy forgings, of the forgings themselves. If that was done, and provided the steel in the first place was of suitable quality, it could be ensured—in fact, almost guaranteed—that there would be little or no trouble. In steel which was peculiarly sensitive to irregular treatment, too high or too low a temperature, then without these precautions undue crystallization or strains would be set up in the forgings produced. To speak still more

(Sir Robert A. Hadfield.)

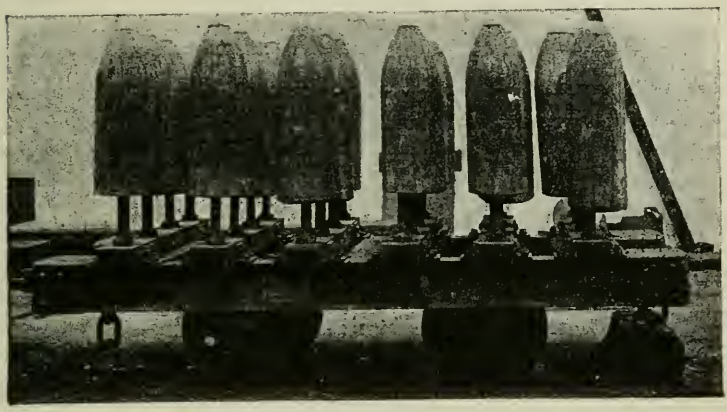
specifically, supposing that the operator started with ingots and not billets or rough forgings, then, specially with harder steels, it was very inadvisable to put cold ingots into a hot furnace. In other words, the temperature of the furnace and the ingot or blank should rise together so as to ensure a slow and consistent heat treatment. His firm had been led to study this question in connexion with the working of manganese steel ingot forgings. He quoted that material, because it was such a very bad conductor of heat.

It was quite possible to abuse ordinary steel without spoiling it, but any abuse of such a special alloy as manganese steel would lead to serious difficulty, on account of its conduction of heat. That was a point to which he thought metallurgists might pay more attention, in order to ascertain the particular kind of conduction in the steel being dealt with. In other words, in the first place, if they knew that the steel was a bad conductor of heat, it was of the greatest importance to treat that steel more carefully during the first heating operations. In the manganese steel mentioned, if not so treated, the ingot would not be long in the furnace before a clink would be heard, or, if that was not heard, cracks would be there which would show themselves later on, the result being spoilt and waste material. Even, however, with such special steels as manganese steel, nickel chromium and other steel, provided care was taken to heat very gradually the object which it was desired to forge, success could be readily obtained. In order to show how critical a point that might be in the special steels to which he had referred, a good plan was found to be that of allowing the ingot to be heated near the furnace itself, so as to raise it to a temperature of perhaps 200° or 300° C. This would give a better chance of success afterwards.

In the same manner cooling down should be most carefully watched and supervised. In many cases, except in very soft steel—and even then—it was dangerous to take forgings and allow them to cool in the open air without proper attention to that detail. As an illustration he would show a photograph, Fig. 19, which represented certain important forgings which, cooled in this way, did so uniformly and without becoming embrittled, as might

otherwise be the case, or too much softened, if all were thrown into a big pile or heap. In making these forgings for a "common or garden" article, whose name he would not mention, although it was familiar to all present, a small trolley with upright pins was used as shown in the photograph. The blanks were placed on the top of the pin, some fifteen or twenty being placed on one trolley. By that means no one blank touched another, and consequently they cooled down much more uniformly and evenly. As a result he was not aware of any failure in tests of such forgings made by his firm, although hundreds of thousands of such articles were being

Fig. 19.—*Cooling Steel Castings.*



turned out. This was, he believed, due to the great care taken to see that the cooling down was made uniform, in the simple way mentioned.

No doubt many of those present had often heard of the term "normalized" being used, which was really a very proper attempt to produce steel which had been heated to a known and reasonable temperature and cooled in a known and definite manner and time. Incidentally, as proving the care with which steel could now be treated during its process of manufacture, and although the facts referred to did not quite bear on the question of forgings, he might mention that in America, where they were beginning to pay much more

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attention to the important question of supervision of temperatures, a large steel works department took the temperatures of several hundred heats of fluid steel by optical pyrometer, and found that, by proper supervision and control, its temperature did not vary more than 30° C.—rather a remarkable result, seeing the small difference at such a high temperature as that represented by fluid steel, somewhat about 1,500° C. That was an excellent result, in view of the many variations which occurred, and which required careful control and supervision. Incidentally the necessity for taking care in that respect applied equally to forgings, and it could not be emphasized too strongly that those who handled forgings, whether small or large, should most carefully pay attention to pyrometer observation, whether optical or resistance.

On the question of heat treatment subsequent to forging, Mr. Ashdown had put forward a number of interesting points. In that respect he would like to say that the ferrous-metallurgist, in one of the branches of his work, had mainly to deal with temperatures and combinations which occurred far above those relating to the ordinary chemical industry. Ferrous-metallurgists were dealing every day with metals and combinations of metals requiring temperatures of from 1,400° C. to 1,600° C. as regards the metals themselves in their molten condition. The temperatures required to bring about that molten condition were of a still higher order, probably up to 2,000° C., or, to the steel-maker using electric furnaces, temperatures considerably over 3,000° C. Even in the lower ranges of temperature, such as used during rolling, forging, heat treatment and other operations, temperatures were high. For example, rails were rolled at from, say, 1,100° C. or 1,200° C.; and many other instances could be quoted. Even then the temperature work of ferrous-metallurgists was not done. They had to deal with the same metals at quite another set of temperatures, known as "treatment temperatures," varying from 500° C. to 900° C.; and finally, in some cases, when tempering was necessary at still another and lower set of temperatures. In all those matters they needed the guidance, care, and knowledge of the engineering chemist and physicist. The non-ferrous metallurgist

on the other hand, stepped down considerably in the average temperatures he used for fusion, rolling, forging, and other operations, thus requiring still another type of experience and training.

It had often been stated that constant vibration would set up a particular kind of crystallization in a steel article. His own experience, however, was that steel properly treated could not break in that manner. Moreover, the old theory that large crystals were formed in improperly treated steel after being put into use was quite incorrect. Large crystallization, as seen in many fractures, was there because the original structure was coarse through overheating chiefly. In other words, vibration was only developed from what was originally a defect or disease in the steel forging or other article when first produced. As further confirmation of that fact, one might compare the number of fractures in motor-car parts to-day with what happened when the motor-car was first being developed. In those early days forging of special steels was not properly understood. Special or alloy steels were more liable to fracture owing to improper heat treatment than ordinary or plain carbon steel. The result then was that much of the steel produced was in that respect imperfect. To-day, however, now that heat treatment was better understood and temperatures under control, there was very little liability to trouble of that kind, and consequently there were very few fractures. He knew it had been a common case to quote the stems or cam-shafts of stamp batteries which were subject to vibration day in and day out, but he would undertake to say that if properly heat-treated these would never break from that cause, always provided, of course, that the stress on them did not exceed the elastic limit, which should not be the case if the shaft was of a proper design and section to take up the stresses.

There was only one other point to which he desired to refer in connexion with the question of heating. Even with the very best gas-fired or coal-fired furnaces, it was impossible to get uniform temperatures, such as were absolutely necessary for dealing with steel, which was a much more delicate material to heat and handle

than most people thought. He hoped that before long some method would be found for employing electrically-heated furnaces. There was no doubt that with such furnaces more uniform control could be obtained and variation of temperature avoided. He believed that much was being done in that respect in America, and he hoped that this country would not be behind in dealing with the matter. Mechanical engineers ought to be able to step in and help to produce still better electrical machinery, so that power could be obtained more cheaply. He hoped to live to see the day when not small laboratory furnaces, but large electrically-heated furnaces would be in use in this country, by means of which it would be possible to produce a more uniform and better quality of steel, simply because the crystallization of the steel could be controlled in an easier manner. In conclusion, he desired to return his thanks to both the Authors for their very able Papers. In particular, he thought the Institution was very favoured in having received such an excellent Paper from the President of the Iron and Steel Institute—it at least showed that there was no jealousy between the two Institutions.

Dr. Walter Rosenhain, F.R.S. (National Physical Laboratory), said the Papers were extraordinarily interesting and important, and it was a pleasure for him to be able to say at the outset that personally he felt thoroughly in agreement with nearly everything that both of them contained. As one who was essentially a laboratory worker, he desired to say that he had never been under any delusion as to the identity of operations in the laboratory and in the works but he thought most intelligent laboratory workers were aware, as he was, that there was a definite relation between the two things. If a certain structure could be produced in the laboratory, then, provided the identical rate of cooling or heating could be made to occur in a forging or a portion of a forging, identical results as regards microstructure and properties would be obtained in a large body. In that sense he thoroughly agreed with Mr. Ashdown's statement that the results in a laboratory could be completely reproduced on a large scale; but the difficulty lay, as Sir Robert

Hadfield had pointed out, in getting the rates of cooling and in getting the steel to stand them. A bar 1 inch in diameter quenched in water might stand without cracking, but if an attempt were made to produce the same rate of cooling in a steel forging 3 feet in diameter the result would probably be disastrous; it could not be done. In that case a method of using the different kinds of steel (particularly alloy steels) must be adopted which gave the desired structure with a rate of cooling attainable in the works There was one point in Sir William Beardmore's Paper that he desired to refer to in that connexion, namely, the method of treating forgings consisting of portions of very unequal sizes. The method Sir William suggested for treating such pieces of very unequal sizes might no doubt be satisfactory, that is, applying a heat insulating coating to the thin parts and thus retarding their heating.

Another way, which was possibly easier—and also practicable, because he had seen it done—consisted of the following method. When the whole of the forging had been heated to the temperature from which it was desired to cool it finally, that is, the proper temperature for heat treatment, the thin pieces at the end had been over-heated; they had been exposed to the temperature for too long a time, as had been pointed out in both Papers. Supposing now the forging was taken out of the furnace, or put into a cooler part of the furnace, and was allowed to cool down so far that the thin portions were actually below the critical point, that is, somewhere a little below 700° C. The transformation of the steel would then have taken place in the thin parts, and in the outer layers of the thick part, but not in the central portions of the thick part, because that would not have cooled down; that would still be well above the critical point. He suggested that when the forging had been cooled down that far it should be returned to the furnace and heated up again long enough for the thin parts to rise above the critical point. By that time the outer layers of the thick part would also have risen above the critical temperature, and the whole forging would be in a uniform condition. It could then be heat-treated at a uniform temperature, since it would then be certain that the time which had elapsed since any part of it passed

(Dr. Walter Rosenhain.)

above the critical point, which was what really mattered, was uniform right through. That was a method of treatment which was perfectly rational and practical, and, with a very large forging, which a steel-maker could afford to handle individually, might prove easier than other methods of protecting the ends. That was the rationale of what was frequently specified as a double-heat treatment, which was often used in regard to small forgings, and it had a similar effect in large pieces. It meant that the outer portion had been heat-treated in a most satisfactory way, whilst the inner portion had also been properly treated.

There was one point in regard to which he wished very seriously and very emphatically to sound a note of warning, although he knew that practical steel-makers might perhaps at first be a little sceptical in regard to it. Nevertheless, he desired them to pay the greatest attention to the point. He referred to the injury which could be done, and was done in many cases, to steel by prolonged heating below the critical temperature—that is, at temperatures between 600° and 700° C. He desired by means of a few lantern slides to illustrate what happened to steel under those conditions; but before doing so he wished to draw attention to the fact that the structures which Sir William Beardmore and Mr. Ashdown had illustrated related to the distribution of ferrite and pearlite in the steel. But the condition of steel was dependent upon something beyond that. The structures illustrated in the Papers were merely arrangements of ferrite and pearlite, but if the dark constituent of those photographs were examined under high magnification it would be seen that it possessed a fine duplex structure. In some cases it was so fine that the most powerful magnification would not reveal it, but it was essentially a duplex structure of ferrite and cementite. They were dependent in regard to the mechanical properties of steel primarily, in large measure, upon the relative distribution of ferrite and pearlite, but they were also dependent upon the inner structure of the pearlite itself, and that could be completely ruined by treatment below 700° C.

Fig. 20 (A), Plate 9, illustrated the typical structure of pearlite under very high magnification. It was a beautiful laminated structure

with alternate fine layers of cementite and ferrite, the bright layers being the cementite, which was the carbide of iron, and the dark layers being practically pure iron. That was the condition in a fairly slowly cooled steel, but pearlite increased in strength and toughness the more intimately those two constituents were mixed. One of them was ductile and tough, and the other hard and brittle, and the more closely they were intermingled the firmer and stronger the constituent became.

Fig. 20 (B) showed another example of pearlite at a high magnification, illustrating the distribution of pearlite and ferrite in a moderately quickly cooled steel. The amount of carbon was immaterial for the purpose; that merely affected the relative amounts of pearlite and ferrite. It would be seen that the pearlite consisted of laminæ, like those shown in Fig. 20 (A), but on a very much smaller scale. If that same piece of steel were heated for quite a short time at a temperature between 650° and 700° C., a striking change would occur, and the nearer 700° C. was approached the more rapidly would the action occur. What happened was that each of the laminæ broke up into a little spherical globule, a process taking place which Professor Howe had named a little whimsically as the "divorce" of the ferrite from the cementite in the pearlite. The two became separated, and the cementite was rolled up into little globules. That was to be seen in Fig. 20 (C), which represented one of the little pearlite areas, or what had been a pearlite area, but it was now broken up into little globules of cementite. The result was that, whereas formerly the laminæ of ferrite and cementite were interlocked, and the whole mass of pearlite resulting from that interlocking was itself interlocked with the ferrite crystals and served as a support and a source of strength, in the present case the cementite was nothing more than so much dust scattered through the steel. It was practically valueless as a strengthening agent. On mechanical test, it would be found that steel in this condition possessed properties which would be expected from ferrite not supported by pearlite at all; one found a very low elastic limit and an extraordinarily low resistance to shock. Unfortunately many of the ordinary tensile

(Dr. Walter Rosenhain.)

test specifications would pass such a steel without question, particularly if it were hardened by mechanical treatment, because the particular effect which was produced by this injurious treatment was not very evident in an ordinary commercial tensile test. The yield-point was found slightly lower than would be expected, but everything else, the ultimate stress and the elongation particularly, was satisfactory; the moment a shock test was applied, however, or the true elastic limit determined, a very serious state of affairs was found. That could be remedied at once by heating the steel for only a few seconds above the critical temperature and then cooling it down at any reasonable rate.

Mr. Ashdown had spoken rather slightingly of the term "fatigue." Personally he knew very well that it was used to cover a multitude of ignorances; but at the same time it was the kind of thing to which he had just referred, and other kinds of bad heat treatment leading to a low elastic limit, which resulted in fatigue failures. So long as the stresses to which an object was exposed did not exceed the true elastic limit, as Sir Robert Hadfield had pointed out, there would be no fatigue failure. What had to be looked for was that wrong treatment of that kind seriously lowered the elastic limit and thus gave rise to local deformation under slight stresses, resulting in the final disintegration of the steel.

Another danger which attached to a heat treatment below the critical point was that, as had been quite recently shown—within the last five or six years, at all events, and more clearly quite recently—if steel which had been slightly cold-worked, that is, subjected to mechanical deformation at a point below its annealing temperature—it need not be quite cold, it might be at a blue heat —if the amount of deformation happened to fall within certain limits, and if it were then annealed at a temperature below 700° C., a very curious change took place, which consisted in the rapid growth of the ferrite into large individual crystals during the annealing process. There again a more superficial examination very frequently failed to reveal that state of things, and tensile test as a rule showed very little of it. The figures for the shock tests however, went down to one-tenth of what they ought to be in

properly treated materials. Here, therefore, there was another danger in the annealing of steel at temperatures below 700° C. If steel which was annealed or treated above the critical point was used and allowed to cool down without prolonged stewing afterwards, both those dangers were avoided, and such treatment was a fairly easy precaution to adopt.

He desired, in conclusion, to show Fig. 20 (D) and (E) in reference to the point raised by Sir William Beardmore with regard to the enormous difference in structure which was found between the outside and inside of a large forging. The two photographs illustrated the structure of a turbine-shaft which failed in use and which was afterwards examined. The two photographs were on the same scale and indicated the great difference in the size of the structure between the outside and the inside of the shaft. Fig. (D) showed the coarse structure of the inside of the shaft, and Fig. (E) showed the outside layers on the same magnification, and the enormous difference between the two structures would be seen.

Dr. W. H. Hatfield (Brown-Firth Research Laboratory) thought that all concerned with the industry were indebted to the Authors for their valuable Papers. With the increasing demand for large masses of steel, it was essentially of importance that the engineer should appreciate the technical difficulties in the way of the steel-maker in the manufacture of such large pieces. He desired to say definitely that engineers might be quite confident of the fact that the steel-maker did know his business. He was inclined to think that Mr. Ashdown had hardly done justice to steel-makers in general, and himself in particular, by the rather diffident way in which he had spoken of the efficiency with which scientific principles were applied in steel-works practice. He was also afraid that the slides Dr. Rosenhain had shown would not enlighten the President very much with regard to the various microstructures presented in large forgings. The large steel-works of the country were very well equipped; they were invariably run by highly trained men, and the value of the two Papers that

(Dr. W. H. Hatfield.)

had been read seemed, from his point of view, to lie largely in the fact that the Authors were expressing, for the advantage of engineers, some of the difficulties which they encountered in their work. In that connexion he desired to say that Mr. Ashdown appeared to have given the impression that it was very easy to produce in large masses the mechanical results which could be obtained in small pieces of steel. That was not very easy. Very great limitations were set upon what could actually be accomplished by the mass that was being dealt with. A very large mass of steel could only be heated and cooled with relative slowness; if it was necessary to pass the critical point, or any specified high temperature, a long time was necessary, and it was highly desirable that engineers should fully appreciate that point of view. Bearing on that subject, he noticed that Sir William Beardmore, in the figures on page 223, showed that for a very large forging made from a 60-inch ingot there was a yield-point of 22·6 tons per square inch, an ultimate stress of 38·8 tons per square inch, and an average elongation of from 24 to 30 per cent. He was interested in those figures, and would be glad to know from the Author whether they represented one particular experimental set of results or whether he considered they represented the average figures which could be obtained in works practice from forgings of that size. The A tests taken longitudinally from the ends of the forgings gave 39 tons, with 27 per cent. elongation, and the curious feature was that the Author claimed to have obtained from tests B and C a similar tonnage with an average of 27·5 per cent. elongation, both cut transversely as regards the direction of the grain (and the position of the drawn-out inclusions such as sulphides and slag). One would have anticipated that the transverse tests would have failed to be equal to the longitudinal ones, apart from the consideration that the latter were cut from the lighter section of the forging.

The statement was made on page 217 that: "For an ingot 83 inches in diameter the maximum size of the forging should not exceed 48 inches for the best practice." He put it to the Author that without a good deal of qualification it was unnecessary and certainly inadvisable to state figures in the way those figures were

stated. The inference was that 83 inches in diameter was a minimum size of ingot from which a 48-inch mass could satisfactorily be obtained, whereas marine engineers in particular, who dealt with very large masses, knew there were occasions when that suggested ratio might possibly not apply. Bearing on that point, he desired to emphasize that it was not only the relation of the size of the ingot to the finished forging which must be considered, but also the technique. For instance, the temperature at which a mass of steel was forged would have a great influence not only on the final mechanical properties of the steel, but also with regard to the uniformity with which the work was taken up through the mass. He quite agreed with Sir William Beardmore in the statement made on page 218: "In this connexion the cause of commercial efficiency would be better served if engineers, in bringing out new designs in heavy steel construction, would consult the metallurgist and steel manufacturer as to the best method of using their combined knowledge in the design and production of special requirements." He was sure that all steel manufacturers would be only too glad to place before engineers their experience, and any data they possessed which might in any way conduce to success and safety in the final forgings.

A reference was made in Mr. Ashdown's Paper to the subject of fatigue, and in that respect he heartily agreed with the remarks that Sir Robert Hadfield and Dr. Rosenhain had made in regard to it. The word "fatigue" was indefinite and meaningless to technical men who had really to study that phase of the subject. He quite agreed that if the elastic limit (not yield) of the material was carefully determined, stresses within the elastic limit would not result in rupture. He must, however, qualify that by saying that they must not even locally exceed the elastic limit, because it would be readily understood that any irregularity on the surface or any minute surface defects would cause a concentration of stress which would rush up the stresses in that particular locality well above the elastic limit, and that, he believed, was largely the explanation of the numerous instances of fatigue that were heard about. Just as he had suggested to Sir William Beardmore that it was

(Dr. W. H. Hatfield.)

inadvisable to state specific figures in regard to the size of ingots for forging, so he suggested to Mr. Ashdown that it was unwise to state that the photomicrograph shown in Fig. 7, Plate 5 (medium carbon steel), illustrating the "drastically overheated" condition, represented invariably the drastically overheated condition of steel. He suggested that structures of that kind were met with pretty largely when the steel had not been overheated.

He did not agree with Dr. Rosenhain's suggested method of heating up unequal masses, and he thought Dr. Rosenhain would withdraw his suggestion after a careful consideration of the actual operation. Imagine the difficulties introduced in withdrawing a large forging with a view to cooling in the manner indicated, the cooling down of the furnace and the general organization induced, from the standpoint of accurate working. He (Dr. Hatfield) agreed with Sir William Beardmore and Mr. Ashdown in that matter, and, of the two, preferred Mr. Ashdown's view. Dr. Rosenhain also stated, or appeared to state, that annealing below the recalescence point was inadvisable, but he did not think the reasons Dr. Rosenhain gave justified him in taking up that point of view. Anyone with considerable experience in the handling commercially of those steels must know that there was quite a lot to be said for low-temperature annealing.

In conclusion, he desired to refer to the remarks made by Mr. Ashdown to the effect that the difficulty of the application of science to industry was a severe one, and that something would have to be done to enable works to make better use of scientific knowledge. He put it to the Author that the only way to overcome the difficulty and to enable works to take full advantage of the scientific work that was being done in the country, was to see that the young men in the works were thoroughly trained on a somewhat similar plan to the men who were doing research work. These lines had been followed with very good results for some time past, both at Messrs. John Brown and Co., Ltd., and Messrs. Thos. Firth and Sons, Ltd. It seemed to him that very little difficulty would be experienced as long as a broad view was taken and not too much was expected. If the staffs in the works were made

thoroughly acquainted with the scientific principles, he was sure excellent results would be obtained.

Dr. H. C. H. Carpenter (Imperial College of Science and Technology) desired to refer to the statement in Sir William Beardmore's Paper, which had already been very briefly alluded to by Dr. Hatfield, that, in order to obtain the best results in a forging, the cross-section of the ingot should be so reduced that the ratio of its cross-section to that of the finished forging should be 3 to 1. He thought it was desirable for the members to realize what the implications of that statement were. The larger the ingot, the greater would be the difference in composition, particularly as regards carbon, sulphur, and phosphorus, between the outside and the centre; in other words, the greater would be the tendency to segregation. Secondly, the larger the size of the ingot, the greater would be the contraction stresses set up. Thirdly, the larger the ingot the greater the difference between the size of the crystals constituting the skin and those in the axis. All of those were drawbacks, and one of the reasons for the carrying out of a considerable amount of forging upon the ingot was to get rid of these undesirable qualities set up in the metal. He therefore thought the statement that the cross-section of the ingot compared with that of the bar should necessarily be 3 to 1 ought to be accepted with caution. He desired to quote a few figures, in bringing out that point, which Professor Edwards and he (Dr. Carpenter) published in the Eighth Report of the Alloys Research Committee on the Alloys of Aluminium and Copper. Sand castings of a bronze with 10 per cent. of aluminium gave on an average an ultimate stress of 31·70 tons per square inch, and 21·7 per cent. elongation on 2 inches.

The members would realize that Professor Edwards and himself were working on a small scale, but he thought that the tests gave an indication as to what also happened in larger masses of metal. A bar was then cast 3 inches in diameter, and 20 inches in length, and rolled down to $1\frac{1}{4}$ inches, which gave nearly the ratio of 3 to 1 which Sir William demanded. An ultimate stress was then

(Dr. H. C. H. Carpenter.)
obtained of 35·03 tons per square inch, with an elongation of 31·5 per cent—a very distinct improvement in both cases. The bar was then rolled down to $\frac{3}{16}$ inch, which rather exceeded the ratio of 3 to 1, and an ultimate stress was then obtained of 37·7 tons per square inch, with an elongation of practically 30 per cent. That was a still further improvement. The effect of the forging of the 3-inch ingot down to $\frac{3}{16}$ inch was to cause a marked breakdown of and considerable interpenetration between the crystal units of the metal, but exactly the same thing could be brought about in one operation if the alloy was cast in a chill mould. That Professor Edwards and himself showed by casting the same alloy in the form of a bar 1 inch in diameter, and it then gave an ultimate stress of 37·0 tons per square inch with an elongation of 30·7 per cent. The figures for the chill were very nearly identical with those of the rolled bar which had been reduced in cross-section from 3 inches to $\frac{3}{16}$ inch, and the explanation was that by casting that alloy in a chill mould the crystals were preserved very largely in that state of interpenetration which was produced by forging down the large ingot to a comparatively small section.

Mr. J. H. R. Kemnal thought the Papers gave engineers a great deal of useful information, particularly about the heating of steel, but the steel-makers would considerably add to their indebtedness if they gave further information about the cooling of steel. During the war he had had a good deal of varying and wearying experience in regard to the heating and cooling of steel for the making of shells, both large and small, and some of the experience had been of a very curious nature. Repeated instances had been found in which shells, that were cooled in the shop and were subject to a draught, gave different test results altogether from those that were cooled in a protected place. Different results had also been obtained from cooling in summer time as compared with winter time. Many cases had occurred of shells being rejected on that account, so that the point was one of great importance to shell-makers. He imagined that other people who had forgings to make of any kind must have had more or less the same experience, which

extended not only to shells, but also, as was well known, to forgings for boilers. Cases had repeatedly occurred in which forgings had been brittle at one point where they had been cooled in a draught. As the result of his experience and certain experiments that had been made, he thought a good deal of uniformity and reliability in forgings would be obtained if some determined method were adopted of cooling them, not by laying them down in the shop, but by putting them in a closed receptacle into which air was mechanically circulated. That, he thought, would overcome a great deal of the trouble, though it was not a condition that was generally accepted by the inspecting authorities.

One further point he desired to mention was the existence of fatigue, which, according to steel-makers, ought not to occur. He thought the experience of most engineers was that it did occur, and he called to mind at the moment an instance which could hardly be called anything else. In a very large riveting machine which had been at work in his establishment for five years, the power put on the rivet was 100 tons; the riveting post was a forging about 10 feet long, and about 18 or 20 inches in diameter, and after five years' use it broke right through at the point where the greatest stress occurred. On examination, the structure showed no peculiarity as compared with the ordinary structure of commercial steel. If it was not due to fatigue, he would like to know what caused that riveting post to break.

The President, before calling upon Mr. Ashdown to reply, said he thought it was admitted, in connexion with large forgings, that the higher the temperature the quicker the crystals grew. Large crystals were not desired, and therefore the forging must not be left for any length of time at a high temperature. All the time the forging was cooling the crystals were growing; therefore some means must be adopted for breaking them up. That was done by mechanical work, by hammering them or rolling them, whereby the volumes of the crystals relatively to their surfaces were diminished. If hammering was continued until the forging got fairly cool, the time came when the crystals were broken up faster than they

(The President.)

formed. But a time arrived when the metal got so cold that if the hammering was continued cracks developed, just as in a piece of steel unduly strained. What mechanical engineers desired to know was what was the lowest temperature at which a piece could be forged relatively to its size, and what was the point at which mechanical deformation must be stopped. He thought that was really the question, and he would be glad if Mr. Ashdown would answer it.

Mr. H. H. Ashdown, in reply, after thanking the members for the very kind way in which they had received the Paper, said that Dr. Hatfield had suggested that it was desirable that the students and young men should be thoroughly trained in the works, and that his firm were giving a great amount of encouragement to these men. He showed a slide,* referring to students' research work in the heat treatment of steel, which would be of great interest to Dr. Hatfield. Eight students had been through his (the Author's) research laboratory. The actual work itself was done in the works under works conditions, but in order to prove the effect of the heat treatment on the steel, the pieces were cut off and examined in the laboratory, where the final research work was carried out. Some of the figures related to incidents that had occurred many thousands of miles away from here; and in that connexion he desired to sound a note of warning that when the war was over this country would experience keen competition from distant countries where every care and attention was being paid in order to assimilate the full value of research in the works. In one country which the Author had in mind there were many men training very hard to meet that

* The diagram gave the composition of a steel, together with a heating and cooling curve and twelve photomicrographs, showing the results of annealing, quenching, and tempering at different temperatures.

The mechanical tests—giving the yield, tensile, elongation, reduction, and Brinell hardness corresponding with the photomicrographs—proved that finished forged steels, when drastically heated to a temperature as high as 1,200° C., could, by careful heat treatment, be refined and restored to give results quite equal to the carefully finished and treated forging.

commercial competition when the time arrived. One of the pieces referred to in the diagram related to a rolled bar of something like 5 inches in diameter. In addition to that, pieces were taken from marine-engine forgings and forgings of all descriptions, and treated in precisely the same way as the forgings that went through the works in the ordinary course.

He quite agreed with Sir Robert Hadfield's remarks, and thought the members were very greatly indebted to Sir Robert for the valuable information that he had so openly placed at the disposal of engineers and steel-makers throughout the country. Sir Robert appeared to be in full agreement with the remarks made in the Paper, that so-called fatigue was largely due to the crystalline structure of forgings as they left the works.

He, the Author, did not think electrically-heated furnaces would overcome all troubles, particularly in cases where a miscellaneous collection of forgings of widely different sections had to be dealt with together. The President stated that he was hoping to hear the truth, the whole truth, and nothing but the truth. Personally he had hoped that some of the members would have come along with a little more of the truth. The Paper gave the leading points; in fact, he was asked to write the Paper in such a form that it would promote discussion, and he would be very pleased indeed to receive contributions in continuation of the truth which he had already set out in the Paper. He did not think the use of technical terms would be acceptable to practical men in the works.

For some years to come it would be necessary to depend very largely on the practical men, particularly those who had acquired a good technical training, and who were still the backbone of our great commercial institutions. When the next generation followed on, he believed they would be very much better off in that respect—at any rate he sincerely hoped so. It would then be possible to associate technical terms with works practice, because they would be more or less quite familiar to the young men now in training. Inasmuch as metallurgists at the present time were still quibbling as to which were the correct definitions for certain constituents of

(Mr. H. H. Ashdown.)

steels, he did not think practical men could be expected to realize what was meant by such terms. The President, who had raised a point with regard to the size of the crystals, said he considered that the development of crystals occurred during the cooling down and during the forging operations. The size of the crystals depended to a large extent on the final heat treatment that the forging received. It was quite true that the crystals were broken down by the forging operations, but, generally speaking, crystals were not developed under forging conditions, not even when hammered cold. Strains and fine structures were produced under these conditions which promoted cracking, but that did not give rise to the big crystals which were noticed in forgings.

The PRESIDENT said he thought it might be due to slow cooling.

Mr. ASHDOWN said the large crystalline structures occasionally met with were mainly due to long periods of heating at high temperatures, after which it was immaterial whether the forging was quickly or slowly cooled.

He was sorry he could not quite follow Dr. Rosenhain's explanation with regard to breaking down the structures of the body of the forging illustrated in Sir William Beardmore's Paper. He would very much like to discuss the matter with Dr. Rosenhain, because it was one of great interest to him, and he could hardly appreciate how the structure of the internal portion of the shaft was affected in the way suggested. He was very pleased indeed to receive Dr. Rosenhain's very valuable explanation of the separation of the carbon constituents which took place by heating under the change-point of the steel. He did not know whether Dr. Rosenhain was referring to the suggestion he (the Author) put forward, that furnaces charged with forgings should be maintained for some little time under the change-point of steel. Dr. Rosenhain would appreciate that the temperatures were subsequently raised up well over the change-point, so that he presumed Dr. Rosenhain would agree with him that re-diffusion of the carbon constituents would take place. He assumed that Dr. Rosenhain was only referring to

forgings which were soaked under the change-point and which received no subsequent heat treatment.

Dr. Rosenhain said that that was the case.

Mr. Ashdown, continuing, said that Dr. Hatfield had criticized the fine structure produced in the works as compared with those in the laboratory. The forging of which an illustration was given in the Paper was a hollow forging with a 10-inch wall, and he maintained that the same tests could be obtained under modified conditions. If a forging with a 10-inch wall was being quenched, it was necessary to reduce the tempering temperature very considerably in order to obtain comparative results.

Dr. Hatfield said that he was only applying the Author's remarks to the larger type of forging mentioned in the Paper.

Mr. Ashdown said the forging to which he referred was a hollow forging, weighing 30 tons, with a 10-inch wall. He simply gave the photomicrographs as comparisons, and they were actually obtained from forgings. He did not make any statement with regard to the structures or similar structures not being obtained under other conditions. They were simply structures from forgings which had been drastically overheated, and he was more or less responsible both for the overheating and also for the refining.

He thought the late Sir Frederick Donaldson ought to receive full credit in connexion with the training of students in the works. He believed that Sir Frederick was the first to introduce students at Woolwich for technical training, and the plan was then followed by the leading steel manufacturers in the country. He was very pleased to say that he received his own early training under the late Sir Frederick Donaldson, and he was very much indebted to him for the wide experience he had gained. The system of employing technically trained men in the works was not altogether new; at Elswick they had men who received their training at the Royal School of Mines and had since grown grey in the service of the firm.

(Mr. H. H. Ashdown.)

Mr. Kemnal had raised an interesting question relating to the cooling of steels. The variable condition obtained in steels depended entirely on the class of steel that was under treatment at the time, and also the finishing temperature. It depended entirely on the specification which had to be worked to, of the subsequent treatment the shells should receive, and whether they should be slowly cooled or quickly cooled. Many of the troubles experienced could be governed by controlling the forging conditions. If they were to be left in the finished forged condition, then there should be uniformity in, and a definite temperature for heating, and something like a definite comparative result would then be obtained in the finished forging. He (the speaker) thought it was highly probable that the marked difference that occurred in the summer as compared with the winter was due to the difference in light. It was quite likely that in the summer time a much higher temperature was worked to than in the winter, and very marked differences would be obtained in the results, because the finishing temperature would be very much higher. If his Paper had been of any value, the members were indebted to Dr. Stead, who made himself personally responsible for obtaining a Paper giving a practical aspect of the subject.

The President said that in view of the fact that Mr. Ashdown had stated that Dr. Stead had persuaded him to give this Paper, he thought the members ought to accord him a vote of thanks as well as Mr. Ashdown.

The resolution of thanks was carried by acclamation, and the Meeting terminated.

Discussion in Birmingham, 22nd March, 1917.

The Papers by Sir William Beardmore, Bart., and Mr. H. H. Ashdown were read in abstract by Mr. John W. Hall (*Local Correspondent*).

The Chairman (Mr. A. A. Remington), in opening the discussion, said that he wished to express the indebtedness of the Birmingham members of the Institution to the Authors of the Papers. A great deal of information was afforded both by Sir William Beardmore and Mr. Ashdown, some of which was most interesting. Unfortunately, no definition was given of the size at which what was termed a "large forging" might be considered to commence, but he assumed that work of 5 tons and upwards was meant. Although each of the communications was of an interesting character, it seemed to him a pity that each Author's remarks and conclusions had not been made more definite. For instance, they mentioned work being spoiled by excessive heating (described by Mr. Ashdown as "drastically overheated") or by slow cooling, especially in large forgings, rendering recourse to oil-quenching necessary to hasten the cooling, but particulars were not given, in either Paper, as to what temperatures might be regarded as likely to produce "excessive heating," nor were there any exact details as to what might be pronounced as constituting "over-slow cooling." He himself was engaged entirely with productions using small masses of steel, and it was cheerful to be reminded that makers of large steel forgings also had their troubles, many of which appeared to be much the same as those which afflicted such firms as his own. During the last five years or so the branch of engineering with which he was associated had encountered many difficulties in connexion with the heat treatment of steel, and immense improvements had been effected. The result was that at the present time no forgings or stampings were made by them (except minor parts

(Mr. A. A. Remington.)

in bar steel) which were not subjected to some form of heat treatment, either annealing, hardening, quenching, or quenching with subsequent tempering.

He was rather impressed by Mr. Ashdown's criticism of nomenclature as "very vague." The definitions in the British Standard Specifications for Wrought-Steel Automobiles referred to, were only adopted and published after very careful consideration in committee, and, as far as he was aware, they had come to be regarded in the automobile trade as satisfactory, and were being generally accepted.

There appeared to him to be a little misapprehension by Mr. Ashdown in regard to quenching mediums. Mr. Ashdown was an advocate of water-quenching for large forgings, and illustrated the effectiveness of this medium in respect to gun-forgings. The Author went on to tell them that this form of quenching was a good deal in operation abroad, and he commended as highly acceptable forgings so treated which had come before him both for microscopical examination and general testing. He (the Chairman) thought that a good deal of care would have to be exercised before oil could be generally relinquished as a quenching medium. In many cases it certainly would not do to make the substitution. Mr. Ashdown guarded himself by stating that quenching mediums good for large masses were not necessarily the best for light or thin forgings, and this warning he (the speaker) wished to emphasize heartily.

The members would have listened to Mr. Ashdown's remarks on specifications as among the most interesting throughout the communication. It was surprising to read the illustration given respecting rejected high-class forgings, and it was difficult to believe that any inspector would reject a piece of work simply because, after heat treatment, a tensile strength of 37 tons was registered and a sum of 90 representing combined elongation and reduction produced, instead of (as specified) a tensile strength of 30 tons to 35 tons with a combined elongation and reduction of 80. Yet they had Mr. Ashdown's word for it that such an event would occur.

He would like to say a word more upon the vexed subject of specifications. They all knew what serious differences arose between manufacturing engineers and those who placed and controlled the execution of contracts. It had long seemed to him that many of the difficulties associated with specifications arose from the fact that the conditions and terms were drawn up a long time ago, and many of these might be altered, to the great advantage and assistance of manufacturing engineers, without, he believed, lessening in any way the reliability and quality of the finished work—in fact, rather the reverse; and it must be by modifications of specifications to meet improved conditions that producers must in the future look for improvement or relief. To get the manufacturing and inspecting engineers to agree upon or consent to changes was of course one of the engineering problems of the time.

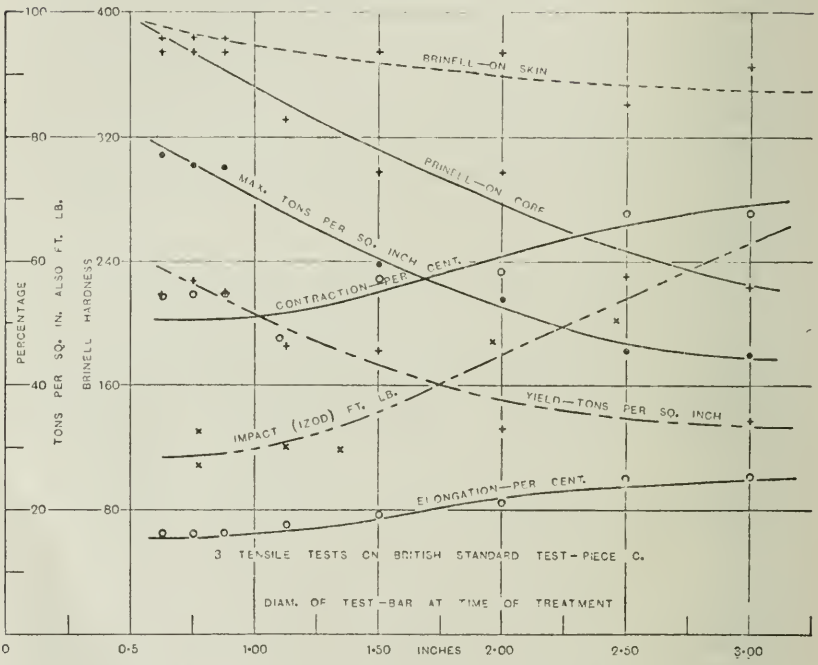
Mr. Ashdown's Paper, in touching upon research work, pronounced that, "it would appear that a gap yet exists between the works and the laboratory, and requires to be bridged across in such a manner that the full value of research investigation can be infused into works practice." Obviously, under these circumstances, the correct thing to do was to educate the practical man in order that he might be put in a position to benefit by the discoveries which research unearthed and placed at his disposal from time to time. While the Author was no doubt correct in pronouncing that the best research work was often lost to the works owing to the neglect to profit from it in the course of actual daily practice, he (the speaker) must repeat what he had said in opening, that in quite recent years vast strides had been made in the production and treatment of steel forgings, though he was prepared to admit that most of the advances had occurred in connexion with small-mass production. He could see no reason, however, why many of the improvements which had been effected should not be applied to large forgings. There were however, of course, directions in which this could not directly apply, and therefore could not be done. The matter of quenching was one of these. He had already referred to this subject, but would add that it was known that quenching mediums

(Mr. A. A. Remington.)

did not have the same result in the case of large forgings, as the effect did not penetrate the mass with the thoroughness which was obtained in respect of small forgings.

The Chairman concluded by exhibiting a chart, Fig. 21, which he had prepared to illustrate the effect of "Mass" on a forging

Fig. 21.—*Effect of Mass on Physical Properties of Heat-treated Steel. Tests taken on Core.*



Analysis:—

Carbon 0·13, Silicon 0·08, Sulphur 0·025, Phosphorus 0·025, Manganese 0·24, Nickel 5·94, Chromium 0·09 per cent.

Heat Treatment:—

Cement at 920° C.–950° C. and cool in box; Reheat for refining to 780° C.–800° C.; Cool in air to 650° C.–680° C.; Quench in water.

respecting the physical properties after heat treatment. The chart recorded the differing physical properties obtained by quenching nickel-steel bars of various diameters from 5/8th inch up to 3 inches. Mr. Ashdown had not given them a concrete example,

so he had thought it worth while preparing this chart to lay before the Meeting. It showed clearly the limitations of quenching effect on the physical properties of the core; the lowering of skin hardness with increased mass was also interesting.

Mr. Frank Somers, having had some experience of heat treatment of large forgings, said it was very interesting to have an opportunity of discussing the Papers which Sir William Beardmore and Mr. Ashdown had read. The heat treatment of large forgings was a most important one, and the more study one gave to this question one found the more there was to learn, and it was more difficult to treat a large forging than a smaller one, as this was due to the difficulty in getting uniform heat in large masses of metal.

The type of annealing furnace also had a great effect on the result obtained, as with large forgings, varying considerably in dimensions, it was essential that they were heated in a furnace which was absolutely under control.

In Sir William Beardmore's Paper (page 218), he gave illustrations of tests of disks which would require to be revolved at a high speed, and on page 221 showed how important the effect of work was on carbon-steel wheels. Although it was rather going away from the question of heat treatment, yet he agreed that the method of forging had a great effect on the tests. The flow of metal due to forging was one deserving consideration, but here he differed from the Author as to the best manner in producing the disks in question. He would have thought that it was essential, in making a disk required to revolve at a high speed, that it should be forged in such a manner that the strength circumferentially would be as uniform as possible round the whole diameter. From the disk, as made in the manner shown in the Paper, at one point of the circumference one would get a longitudinal test, whilst in another position, 90° from the longitudinal test, one would get a cross test. In making disks at the works with which he (Mr. Somers) was connected, it was the practice to forge the disk from a piece which had been cut off from an ingot in such a manner that the periphery of that wheel when finished was of uniform strength,

(Mr. Frank Somers.)

The Chairman had remarked upon the different treatment needed in dealing with large masses of steel, as compared with smaller forgings, and he (the speaker) quite agreed with him in this respect. In shell manufacture, quite recently it was found that in making different sizes of shell, from steel of the same composition there was a great difference in the test of the larger shell as compared with the smaller. Due to the extra thickness of the walls to the larger shell they required more rapid cooling than the thinner wall shell of smaller dimensions, if it was desired to keep up the yield-point. This was owing to the fact that in the larger shell the extra thickness of the wall retained the heat for a much longer time, unless the shells were isolated and put in the cool atmosphere.

Mr. Ashdown drew attention (page 227) to a forging which cracked in the lathe during the operation of turning, due to having been subjected to a number of prolonged forging heats, and attention was also drawn to the fact that on investigation it showed that heat treatment had been at fault. It was a well-known fact now, that it was an advantage to finish a forging, if possible, at one heat, as less stresses were set up in the forging during manufacture.

Referring to Mr. Ashdown's statements respecting inspectors, and that in some cases the inspector rejected the material which, although it might not be up to specification, was quite equal to anything that would conform to the specification, from experience with some inspection departments, this was not so much the fault of an inspector as the department, as there was absolutely no latitude at all given to some inspectors, and unless the material conformed absolutely to the specification, the test results were not passed.

With regard to Mr. Ashdown's remarks about "ghosts," he (Mr. Somers) only hoped they might be the means of getting inspectors to look upon their presence without any unnecessary degree of alarm.

Mr. R. C. V. WHITFIELD said that both Papers were remarkable, not so much for what they said as for what they omitted to say.

Sir William Beardmore, referring to the heat treatment of two large carbon-steel forgings, made no mention of the temperature at which the forging of the several pieces took place. This was really a most important temperature. In practice, it would be found that the forge-man would always forge at as high a temperature as possible, which, as was well known, would tend to the formation of a large coarse grain, with consequent brittleness. He (the speaker) submitted that the reheating to 780° C. (in the one case) and to 900° C. (in the second case) was advantageous, but such advantage was in great measure neutralized by the oil-quenching, which produced hardness, and the final heatings to 550° C. and 640° C. respectively were simply to remove that hardness. If the first heatings had been made and the pieces cooled down normally, the test results would have been satisfactory, and therefore further heatings were unnecessary.

Some few years ago he had considerable experience in the heat treatment of large masses of forged and rolled steel, and many methods were tried in the attempt to obtain the best results. After much experiment they came to the deliberate conclusion that most advantageous results were obtained by a first heating of the material at such a high temperature that the steel was over-heated, that is, heated to that point where there was found an extremely coarse structure with incipient separation of the grains—what Mr. Ashdown would term "drastically over-heated." The over-heated steel, after cooling down, was simply reheated to a temperature between 800° C. and 900° C., varying with the carbon content, and mechanical tests and photomicrographs indicated that the steel was better after such treatment than in the normal condition. In his opinion the many reheatings given by Sir William Beardmore were of no benefit beyond removing the hardness induced by quenching.

Mr. Ashdown was rather severe upon manufacturers on the matter of "fatigue," a phrase which he (the speaker) suggested was employed by the engineer to cover a very wide field of inherent troubles, and Mr. Ashdown apparently suggested that manufacturers were at fault for not having properly annealed their forgings before

(Mr. R. C. V. Whitfield.)
sending them out of the works. His own experience was that "fatigue" so-called was uniformly due to the coarseness of the grain of the material, with consequent reduced cohesion, and that this coarseness could be produced by vibration and similar causes. It was certain, however, that a simple heat treatment at about 800° C. would do much to remove this trouble.

Mr. Ashdown, dealing with "ghosts," suggested that these could be reduced, though not entirely removed, though treatment was costly. His experience was that "ghosts" could generally be traced to the steel-maker—not to his carelessness, but to the "speeding-up" of production, and considerable improvement would be found if a longer period were allowed for complete segregation in the mass. At present he was afraid that too short a time was frequently allowed for the solidification period. He quite agreed with Mr. Ashdown that "ghosts" could not be removed by heat treatment, and the only solution for their removal was to cut out the segregated portion; this often resulted in scrapping the forging.

Mr. A. S. Chew said he had been much interested in listening to the last speaker's criticisms of that portion of Sir William Beardmore's Paper which dealt with tempering temperatures. He gathered that Mr. Whitfield regarded the original heating as all important, and considered that the subsequent tempering had no effect upon the ultimate mechanical tests. He (the speaker) contended that, as an example, if steel were quenched from, say, 900° C., anything might be looked for in the mechanical tests, say, for argument, a tensile stress of 60 tons. Reheating of the same piece at a further temperature of, say, 640° C., would effect another change in the mechanical properties, which would show itself in a reduction of the maximum stress and an increase of the elongation. To his mind this proved sufficiently that the original quenching was the important one that makers of steel forgings need pay much attention to, to be sure of good results. In this particular, therefore, Mr. Whitfield and himself were in agreement, but he could not understand Mr. Whitfield's assertion that no change took place, mechanically, after subsequent tempering. If such were the

case, all research work had been wasted in effecting tempering temperatures, which would enable steels of a known composition to conform to a series of mechanical specifications.

He was afraid Mr. Ashdown's information relating to annealing in sand (page 231) was rather old. His own experience was that if a steel which was too hard to machine was put in sand in the manner suggested in the Paper, the deterioration in the rate of cooling would be insufficient to comply with the requirements of the machine-shop. That this was so had been actually demonstrated by practical difficulties. If normal cooling was too rapid to meet the requirements of the machine-shop, it would be necessary to cool the steel slowly in a furnace, and in many cases it was found best to seal the furnace as nearly hermetically as practicable. The other method described, of quenching above the critical range and tempering back where bulk or mass permitted, was quite the best procedure, in the speaker's opinion.

Referring to Mr. Ashdown's remarks upon high temperature quenching and high temperature annealing, where he stated that he had never found it necessary to exceed either for annealing or quenching, when treating forgings in the mass, a temperature of 840° C., and that he "had had no reason to regret the results obtained," the Author invited an explanation from members of the reasons why some large firms were favourable to high temperature quenching. Whether or not the temperature of 840° C. was sufficient, depended in his (the speaker's) opinion upon the composition of the material. If the decalescent point of the material was above that intended by Mr. Ashdown, then assuredly a higher temperature than 843° C. would be necessary. Sometimes the decalescent point might reach above 900° C. If such material were quenched at a temperature no higher than that allowed by the Author, he was certain the desired result would not be attained —indeed, the object in treatment would be absolutely annulled.

Mr. J. A. Hannay said that he had been somewhat disappointed with the nature of the Papers. It seemed that, so far as those Papers dealt with the subject, makers of small forgings had not so

(Mr. J. A. Hannay.)

much to learn from the producers of large forgings as was sometimes popularly asserted. There must be a great amount of guesswork in dealing with large forgings, if what they had heard that evening was a fair sample. From Mr. Ashdown's Paper it seemed that he placed very little value on the employment of pyrometers, and he was particularly "down" on "recorders," suggesting that their readings were often quite unreliable. The speaker regarded the use of recording pyrometers as essential in modern practice, if it was desired to keep furnaces at a constant and predetermined temperature—a matter generally of very vital importance. He thought that many manufacturers were now of the opinion that recording as well as direct-reading pyrometers were essential: the recorders to enable the managements to know what had been happening in the furnace, and the direct-reading pyrometers situated close to the furnaces to enable the firemen to regulate and keep the furnaces at a proper temperature. Yet Mr. Ashdown seemed, if he might say so, to almost brush temperature-recording instruments on one side. The Author gave an instance of what he termed "the deception" of pyrometers, and he would be glad to have had other examples furnished them. The Author illustrated in this connexion that he had seen "comparatively small engine parts of which within a length of 3 feet, after heating and quenching, one end was as soft as possible while the other end was practically unfileable," and the example was given to show the alleged unreliability of heating instruments. He thought that if while this work was in the furnace, it would have only been necessary for the furnace-men to look inside the furnace to see that the temperature was not uniform at each end of the shaft. It was true that neither of the Authors of the Papers was from Sheffield, but he could not refrain from remarking that on more than one occasion even Sheffield steel-makers, who had visited the Birmingham district to witness the carrying out of their specifications at local works, had been surprised at the practice of makers of small forgings.

The CHAIRMAN (Mr. A. A. Remington), in replying upon the

discussion, said that in the absence of either of the Authors it was impossible to reply to the criticisms which had been advanced, some of which were rather severe, particularly upon the matter of annealing and quenching temperatures. The question of "ghosts" in forgings, as referred to both by Sir William Beardmore and Mr. Ashdown, had been touched upon. As far as he knew, "ghosts" started life in the cooling of the ingot, and he had never heard of their being subsequently removed entirely. He would refer members who desired more information upon "ghost" troubles to recent communications by Dr. Arnold and Dr. Stead, some of which would be found in the Proceedings of the Institution and others in the Transactions of the Iron and Steel Institute.

Mr. Whitfield had touched upon the question of "fatigue." So far as he (the Chairman) knew this was a topic which had been dealt with very little in recent steel literature. There was a very good exposition in a Paper read last year by Mr. J. H. S. Dickinson before the Institution of Automobile Engineers, to which he would refer Mr. Whitfield in particular for further information.

Mr. Chew had referred to Mr. Ashdown's remark upon high temperature quenching and high temperature annealing, and had questioned the accuracy of the Author's recommendation that no higher temperature than 840° C. was ever necessary for treating large forgings. Mr. Chew seemed to carry a good many of the audience with him in sympathy with his remarks. It was difficult to tell upon what grounds the Author had established his thesis, and it would have been very informing could the Birmingham members have had the advantage of his presence. In his absence they could only conjecture. In these circumstances he found himself in accordance with Mr. Chew's criticism that, if the recalescent point of the material was above that mentioned by Mr. Ashdown, as it well might be, then certainly a higher temperature than 840° C. would be necessary for the quenching. It seemed to him perfectly obvious that the recalescent point of steels must be taken into consideration in fixing a temperature from which quenching should occur.

It was not within his function as Chairman to follow Mr. Hannay

(Mr. A. A. Remington.)
in his remarks respecting what the Authors might be able to learn in the Birmingham district from the producers of small work, if they would go to the trouble of making themselves acquainted with what was going on in the Midlands, but he was sure he might be excused if he went so far as to say that he thought Mr. Hannay had not gone beyond the truth, and he noticed that the Meeting and the speaker were very much in accord.

Mr. Frank Somers proposed a hearty vote of thanks to the Authors. The subject of the Papers was one of the utmost importance, and if the effect of these Papers resulted in the Birmingham members engaging upon more research work, the communications would have been of much benefit.

Mr. A. E. A. Edwards seconded the vote of thanks. The Papers had been instructing, and so had the discussion. The practical men who had spoken during the evening had almost unanimously agreed that the Papers could hardly be voted complete, but it was only fair to the Authors to remember that they had had no opportunity of replying to the discussion. As Mr. Somers had suggested that the subject was one which would well repay further research work, no doubt when the country was less busy than now some very pertinent investigations would be proceeded with.

Lieut. W. R. Parsonage, R.N.V.R., in moving a vote of thanks to the Chairman, gave his support to Mr. Hannay's remarks in defence of the useful service of pyrometers. He noted that the Siemen's Water Pyrometer was stated to have its sphere of usefulness. This pyrometer had done much useful work, but he looked upon it rather as an historical than as a modern instrument. He would be interested to learn the reasons for the use of the instrument nowadays. He expressed his concurrence with the Chairman and Mr. Somers that for an inspector to reject a piece of work for so small a variation from the required specification as Mr. Ashdown had mentioned, notwithstanding that the reliability of the forging must have been thereby increased, was surely an exceptional occurrence.

Mr. W. H. Thornbery, who seconded the vote of thanks, expressed his great satisfaction that the Institution was adopting a policy of decentralizing its interests and operations. It would be found a great advantage to supplement the Meetings where Papers were first presented in London by provincial gatherings where country members might also have an opportunity of joining in discussions. It was beyond question that the Papers presented to the Institution were fully as valuable to country members as to those residing in the London district. To-day research work was of greater value to the profession than ever, and the results of investigation and experiment could not be too conspicuously brought before the members in all parts of the kingdom. On these grounds he did not hesitate to say that the decentralizing practice which the Council was now putting into operation would be found to work to the very best interests of the Institution, not only in the matter of increasing its membership throughout the country, but also in respect of the even more important consideration of adding to the members' knowledge and ability in the various responsible positions that they occupied. He felt that in the Birmingham district the step would be found very popular, and he hoped that it would lead to a considerable expansion in the membership of the Institution.

He could not add much to the discussion, since his practice had not been with large masses of steel. There was, however, one point which struck him with regard to Mr. Ashdown's remarks concerning pyrometers. The Author laid much emphasis upon the necessity for seeing that the readings were carried out with the greatest accuracy, if reliable results were to be obtained. He entirely agreed, but it had occurred to him that even if the instruments themselves were of great precision, the results were not always trustworthy, even when the same amount of care was exercised. It must not be overlooked that the instruments were only useful in indicating the temperatures just in that part of the furnace where they happened to be placed. Therefore, to ensure that a uniform temperature was maintained in all parts of the furnace (which had been shown to be so important a point if trustworthy

(Mr. W. H. Thornbery.)
forgings were to be secured), particularly with respect to large forgings, it would appear that frequent readings of the pyrometers would be necessary, and that these readings should be made at different points of the furnace.

The Chairman, in acknowledging the vote of thanks, said he regarded the attendance as very encouraging.

The Meeting then terminated.

Communications.

Dr. J. H. Andrew (Manchester) wrote that he was exceedingly interested in Mr. Ashdown's Paper, which was an excellent survey of the methods employed in the heat treatment of steel forgings. He was somewhat at a loss to understand whether the Author was concerning himself with pure carbon-steels only. In view of the statement on page 227, that "the temperature of 650° C. (1,200 F.) is, as all know, well under the change-point of steels in general," the writer rather concluded that this was so, whilst other remarks in the Paper led him to think otherwise. In the first place, attention must be drawn to a slight error in the Author's interpretation of the term "normalizing." In the standard specifications referred to by Mr. Ashdown, the term was defined as follows: "Normalizing means heating a steel (however previously treated) to a temperature exceeding its upper critical range, and allowing it to cool freely in air. The temperature shall be maintained for about fifteen minutes, and shall not exceed its upper critical point by more than 50° C." Thus a normalizing temperature may be anything between 900° C. and 730° C., depending upon the carbon content of the steel. Mr. Ashdown spoke of normalizing at 677° C.

One paragraph in Mr. Ashdown's Paper (page 232, lines 7 to 17) was somewhat misleading. From this, it would appear that the Author was under the impression that a steel, speaking generally, had but one critical point or range. The writer would like to point out that steels with a carbon content between trace and 0·45 per cent. approximately, showed three critical ranges; those containing between 0·45 per cent. and 0·89 per cent. approximately, had two critical ranges. The only steel having but one critical range was that of eutectoid composition, namely, carbon 0·89 per cent.

Again, Mr. Ashdown spoke of the precipitation of ferrite occurring as the steel passed through the critical range. It was quite true that ferrite in a free state, and ferrite only, did separate at a period between the commencement of the first critical range and beginning of the last, but the change which was the main cause of the softening of steels occurred at a constant temperature of roughly 690° C. (1,274° F.). When this temperature was being slowly passed, not only was there precipitation of free ferrite, but a complete breaking down of the gamma solid solution, into its constituents, alpha iron, and carbide of iron. This, of course, only applied strictly to pure carbon-steels, and not necessarily to alloy steels.

The Author commented upon the loose way in which the word "fatigue" was employed to explain almost any type of fracture The writer was fully in agreement with him in his condemnation Fatigue, as properly understood, referred to a state into which a material was brought by continual, intermittent, or alternating stress. Spontaneous cracking of steel could not be said to be due to fatigue, for this was in the majority of cases the effect of internal or molecular stresses, and might not be due in any way to outside influence.

With regard to the question of pyrometers, the case cited by the Author against their use was the very one the writer would cite in their favour. The Author appeared to object to the use of pyrometers because they showed unevenness in heating, whereas the writer used them for that very object. The writer would like to refer Mr. Ashdown to a Paper on the "Power Forging of Chain

(Dr. J. H. Andrew.)

Cables," by Mr. F. C. Coburn (Society of Naval Architects and Marine Engineers, New York, 1916). In that Paper it was stated that in a furnace used for heating chain cables as many as nine thermo-couples were employed, by use of which it was found possible to keep the furnace temperature constant throughout to 10° C. It was rather too much to expect pyrometers to regulate the furnaces, unless of course a thermostat was installed in connexion with them. If they indicated what was taking place in the furnace, surely they served their purpose.

In conclusion, the writer would like to lay stress on the fact that the treatment which was best for one steel, or type of steel, was by no means best for all. It was almost impossible to give in general terms the so-called best treatment for steels. Steel was almost human in its idiosyncrasies, and appeared more so the more one studied it; each composition varied from the rest, each required a different treatment, peculiar to itself, the more complex a mixture became, as a rule, the more susceptible was it to small differences in temperature. True, there were to be found certain family likenesses between certain classes of steel, but even this did not necessarily hold good always. This fact was unfortunately often ignored by the so-called practical man, who seemed to view all steels alike and expected them to respond to the same treatment. Quite true, the scientist was in many cases just as much in the dark as the practical man, but he fully realized this and admitted it. Experience counted for little or nothing when an entirely new steel was concerned. Moreover, the scientist, knowing the difficulties and complications involved in the treatment of steels, especially alloy steels, refused to discuss the matter until he had made himself as fully acquainted as possible with all he had to deal with. If only the practical man would acquaint himself with some of the theories of heat treatment, he would readily appreciate the difficulties that a research man had to contend with. This would lead to progress, whilst failure to do so would lead to nowhere. The writer did not for a moment wish to run down the practical man; he was invaluable, and was in fact the man who "made the wheels go round"; nevertheless, a little grease from the research

laboratory applied judiciously would make them revolve still quicker.

In the opinion of the writer, before any new alloy steel was put into use, its properties, constitution and heat treatment ought first of all to be thoroughly investigated in the research laboratory. This, given adequate resources and an efficient staff, could easily and quickly be accomplished, and might be the means of saving thousands of pounds when the steel in question came to be manufactured on a commercial scale.

Mr. H. H. Ashdown wrote that the general principles embodied in Sir William Beardmore's Paper confirmed those submitted in his own Paper presented at the same Meeting. There were, however, a number of important details in which he did not quite agree with Sir William, who stated (page 215):—"It is well known that, at any chosen temperature, the time the material is kept at its heat, and the time taken to cool down to normal temperature again, have an all-important influence on the grain size," etc. The writer would suggest that the phrasing was somewhat misleading, and should be modified to read "at certain chosen temperatures which exceed the critical range of the steel," etc.

With reference to small forgings, Sir William said, "a simple annealing will put the material into the condition which will give satisfactory and safe results." "Annealing," as now defined, was a heat treatment which might be under the critical range of the steel, and which would not produce the desired results, and in this instance the term "normalizing" would have better conveyed the treatment presumably intended.

With regard to the results obtained from the heavy disk forgings, the writer did not consider the marked differences shown were so much due to the additional work put on the forging, but were in a large measure due to the effective heat treatment through the thinner section. The writer had dealt with large numbers of disk forgings considerably heavier than the one under consideration, and it had been quite noticeable that if the disks were not raised for a considerable distance off the bottom of the furnace in order to

(Mr. H. H. Ashdown.)

give free access of heat, the central portion of the underside would remain comparatively dark for a long period after the remainder of the forging appeared saturated. This might account for the bad results obtained in the middle of the disk as first instanced. It was further noticed that the second and thinner disk received two oil quenchings whilst the thick disk received only one quenching. It was generally known that a second or double quenching was productive of better results. In dealing with such heavy rotor forgings, the writer had found marked improvements result by boring the shaft hole through the middle and rough machining generally before treatment. The saving in fuel and time fully compensated for the additional cost of the double setting up in the lathe. A further point of interest was that, owing to the comparatively high temperature at which such forgings were finished, particularly those of thick section, the structure was exceedingly coarse, and by oil treatment alone the best results were not obtained. It would be found that an intermediate refining heat treatment materially improved the final condition of the forging.

In heat-treating heavy rotor-spindles, the writer would question if the method put forward by Sir William Beardmore was a safe one. By heating up the body and at the same time checking the heat on the shaft ends, it would appear that a good deal of internal stress might result throughout the length of the shaft. Further, in the writer's opinion, it would appear that distortion was much more likely to result by this method of heating than by the subsequent quenching operation, providing the shaft was immersed in the oil in the vertical position. In works where large quantities of miscellaneous heavy forgings were being heat-treated, it was not a difficult matter to include rotor-spindles in the same furnace charges, and by placing the body of such forgings in the direct source of heat, and shielding the shaft ends by other forgings, it was by no means a difficult matter to raise the whole furnace-charge up uniformly, particularly if, as suggested by the writer in his own Paper, the whole of the forgings were first given an initial soaking heat under the change-point of the steel.

The results of tests from the three positions of the rotor-spindle

were remarkably good and uniform, and it would be interesting to learn if these results represented the average figures obtained from a number of such forgings. It would also be appreciated that the tests taken from the body of the shaft coincided with the purer portion of the ingot and corresponded with chill crystals as recently instanced by Brearley.

Dr. Archibald Barr (Member of Council) wrote that the subject dealt with in these two Papers was one of great importance to mechanical engineers, and it was to be hoped that other communications dealing with the principles involved and the results attained in the heat treatment of forgings might be forthcoming. The writer could claim to possess little practical experience in the heat treatment of forgings, but he might be permitted to make some observations on the subject. It was of great importance, in such a matter as this, that the conditions under which the recorded results had been obtained should be so fully stated that these results—and others that would be recorded —might be co-ordinated into a generally useful body of knowledge. The present Papers were a valuable contribution to a full elucidation of the conditions of success in the process of heat treatment, and all engineers must feel greatly indebted to the Authors for placing the results of their practice in the hands of the members of the Institution.

The unital properties (strength per square inch section, for example) of any casting or forging depended not only on its chemical constitution, but on its physical structure, and the latter depended necessarily not only on the form—or relative dimensions of the part—but upon its actual dimensions.

In order that the results recorded in the Papers should be comparable with one another and with the results obtained by others, the writer would suggest that a little more information should be given with regard to the mechanical tests recorded. Thus, in Sir William Beardmore's Paper, some of the tests were given as having been made on specimens having a ratio $\frac{\text{length}}{\text{diameter}} = 10$, and others on specimens 2 inches long. In

(Dr. Archibald Barr.)

Mr. Ashdown's Paper, the proportions of the specimens from which the results given on Plates 4 and 6 were obtained were not specified.

Though the matter had been frequently discussed, it might not be out of place again to point out that two test specimens of similar form—if of strictly similar material—would give similar results as to unital strength, percentage elongation, etc., if tested at the same temperature and at the same rate of loading per unit of area (at the same number of ton load per square inch per minute). But two specimens of different forms might give very different results. Thus a specimen 2 inches long and 1 inch diameter would give very different results from one 2 inches long and $\frac{1}{2}$ inch diameter, even if strictly alike in material. One 2 inches long and 0·2 inch diameter would give the same results as one 10 inches long and 1 inch diameter. One therefore could not judge of the properties of the material, as revealed in a test, if one only knew the length of the specimen. It might be here remarked further that the temperature at which a test was made had a considerable influence, even within such limits as might be met with in the ordinary practice of testing. In some experiments made by the writer on specimens of ductile steel wire, the "strength" was 21·3 tons per sq. inch at 40° F. and 20·5 tons per sq. inch at 70° F.* Again, the rate of loading had a very marked influence upon the results—a quite important influence even within the limits of rates of loading ordinarily adopted in commercial testing.* Indeed, in the absence of any statement of the rate of loading adopted during a test, it was hardly justifiable to give results to more than two significant figures. Thus a strength of 27·2 tons per sq. inch was not characteristic of a given material, it involved the form of the specimens tested; and it was not characteristic even of the particular specimen tested, as a given specimen would give markedly different results according to the rate of application of the load and the temperature.

In marked contrast with this question of test specimens of

* Journal, West of Scotland Iron and Steel Institute, 1910.

like material—in which the actual dimensions were of no importance, but only the form—one had, in respect to the properties of actual forgings and castings, many effects which depended on the actual dimensions. This was referred to in Sir William Beardmore's Paper.

In three chief respects the qualities obtainable in large forgings differed from those that could be got in small forgings :—

(1) The amount of segregation of the constituents of an ingot depended upon the rate of solidification, and the *possible* rate of transference of heat depended upon the size. Even if cooled in a bath of liquid air, a very large ingot would take a considerable time to solidify to the centre. It was therefore not conceivably possible to produce the same qualities as regards chemical constitution in all parts of a large casting as in a small one.

(2) The physical structure depended upon the rate of cooling, and therefore again the actual size of the ingot had an effect that could not be eliminated.

(3) In forging, a large ingot could not have exactly similar treatment to a small one. The difference in the effects of forging on the outer and inner parts of an ingot was very marked when the forging was down under the hammer; much less if forging was done in a press. The effect of pressing was probably very nearly the same in large and small forgings if—as Sir William Beardmore's remarks suggested—a large and a small forging were made from like forms and qualities of ingots to like forms of forgings.

(4) Reheating, quenching, and annealing. In each of these, again, the actual dimensions greatly affected the results attainable, and further, it was impossible to have the same heat treatment at the centre and at the outside of a large piece. One must therefore question Mr. Ashdown's statement on page 234 to the effect that results could be obtained in the treatment of "masses in the works" as good as those obtained in the laboratory. Would Mr. Ashdown state from what part of the 30-ton forging the specimens of which photomicrographs were given in Fig. 18, Plate 8, were obtained? Presumably the specimens were taken from a part near the surface.

(Dr. Archibald Barr.)

It was of great importance, for a study of the question, to have as full a statement as could be given of the dimensions of the ingots and of the forgings with respect to which results were accorded, and further, in each case to know the particular part of the ingot from which the forging was made, and the particular part of the forging from which the specimens—tested or photographed—were taken. Further, it would be of great importance to know the properties of various parts of the original ingot, and of various parts of the forging at different stages of its treatment. Obviously, these would be difficult and very expensive to obtain, and steel-makers could hardly be expected frequently to incur the trouble and expense, but without such information it would be impossible to attain any very accurate and generally applicable knowledge of the effects of various treatments. The effect of actual dimensions on the results obtained with large pieces made it impossible to carry out any complete research, except in the steel works, or at least with the co-operation of the steel manufacturer.

Under the heading "Research" (page 234) Mr. Ashdown referred to the fact that the chemist was usually "not in a position to apply his reasoning into works practice," and that his reports were "beyond the technical range of a good practical man." The remedy was clearly enough indicated. A practical man might be none the less practical if he had been trained in science sufficiently to understand and appreciate the results of laboratory investigation. Such men were easily enough obtained, or prepared, if reasonable facilities and inducements were offered, but a man with such a double qualification should be a man of the highest rank on a works staff, in status, and in emoluments.

Mr. J. Victor Harrison (Cardiff) wrote that in Mr. Ashdown's Paper (page 230) appeared this sentence: "It certainly causes them (the furnacemen) to be more attentive than they otherwise might be to their fire control." In the opinion of the writer, this was the cause of a great many failures in heat-treating steel. Furnacemen, it was true, had a very good idea of what the furnace heat was by long experience, but to guess to within 20 degrees of heat was

almost impossible. He had known men in the steel works who, through this long practice, would be able to say within a point or two what carbon there was in the charge, but when it came to heat-treating this steel to obtain correct results, correct readings were necessary. The writer had recently a very interesting experience of this rough-and-ready method of heat treatment. In a small works they had to prepare a considerable number of test-pieces for tensile and bending tests. The test-pieces were cut from tubes $8\frac{1}{2}$ inches outside, and 6 inches inside dimensions. The tensile-pieces were cut as shown in Fig. 22, and were machined and ground to $2\frac{3}{8}$ inches long × 0·564 inch. The benders were $4\frac{1}{2}$ inches × $\frac{3}{4}$ inch × $\frac{3}{8}$ inch, and were cut as shown in the illustration. The steel was a fairly high carbon-steel, and there was no nickel present.

Fig. 22.

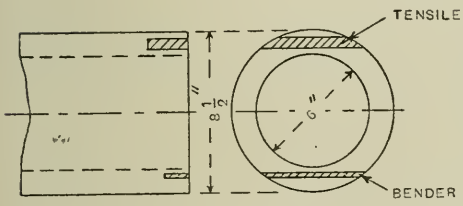
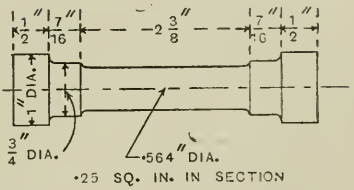


Fig. 23.



On commencing to machine these samples great variation was found in machining. Some were well annealed and cut not unlike mild steel. Others proved almost too hard to machine, and in many cases high-speed milling-cutters broke down in the attempt. There appeared to be spots in the material which somewhat reminded one of a 0·6 per cent. nickel steel. After much difficulty they were prepared for testing. The steel, which was supposed to be well annealed, should have given results of about 43 to 45 tons per sq. inch. On watching the testing the writer was much struck at there being no apparent yield-point. The elongation was practically nil on a length of 2 inches. The pieces, in most cases, broke off short at about 40 to 41 tons, sometimes in the larger diameter of the test-piece, which was $\frac{3}{4}$-inch diameter. The

(Mr. J. Victor Harrison.)

contraction of area was exceedingly small, and there was an absence of a silky appearance at the fracture. He was far from being satisfied, and for a time did not realize what was wrong. The tubes from which these samples were cut were all stamped with cast mark, and it was decided to cut a few samples from the tubes which showed the poorest results. The samples were again heat-treated, to what was considered the correct temperature. On these being tested, results were obtained that a steel of this composition should give.

The question then arose as to what was wrong with the heat treatment in the first case. It was therefore decided to visit the tube-makers and see the furnace. During a conversation with the furnaceman, it was found out that the temperature was supposed to be 600° C. (1,112° F.). The furnaceman was a practical man of long experience, and appeared to know all there was to know about heat treatment and annealing; but that was where he had gone wrong. He was treating a steel quite different from that which he had formerly used. The temperature as per specification was 600° C., and he was asked what was his actual temperature. He replied, "I know it is about 600° C., because I tested it about a week ago." On testing it again in the writer's presence, the temperature was 555° C. To the layman, this difference did not seem a great deal, but it meant, in this case, success instead of apparent failure. The works chemist, in making up this composition of steel, had, no doubt, decided that the heat treatment should be 600° C., and the proof that he was correct was found when the second lot of test-pieces were made, when the heat treatment was 600° C.

The days of "near enough" had gone by, and, as a nation of steel-makers, accuracy must be their watchword. It affected every department in a works, and if it was lacking, costs, etc., went up at once. All this could be avoided by forgetting the old "rule of thumb," and treating the materials in a way as laid down by the chemist. There was much to learn in this direction, and the co-operation of the chemist and the manufacturer would be productive of much good.

In the writer's opinion, the shape and size of steel forgings

offered great facilities as to correct heat treatment. At the present time he had seventy rather interesting forgings going through for rough machining before heat treatment. The weight in the rough was 6 cwt. each, and this after rough machining for treatment was 2 cwt. 3 qr.; he was looking forward to the test results with much interest. The following was the analysis:—

	Per cent.
Carbon	0·50
Silicon	0·157
Sulphur	0·027
Phosphorus	0·024
Manganese	0·760
Nickel	0·542

Great variation had already been found in these forgings, and he was wondering what would happen after heat treatment.

Mr. J. M. Lessells wrote that Mr. Ashdown was to be complimented on his avoidance of scientific terms, because these tended to confuse the issue. He agreed that steel, although it passed the test all right, might ultimately fail on fatigue. It would be quite unfair to blame the treatment or the material, unless one was sure that other factors had not to be taken into account. For instance, the engineer might himself be to blame—(1) for bad design; (2) for a bad choice of material. Of course the steel-maker was always a convenient person on to whom to shift responsibility. He thought that manufacturers of forgings and stampings would appreciate the remarks of the Author as to the necessity of finishing off at a low temperature. Perhaps it would have been advantageous had he stated that stampings made from air-hardening steel offered an exception and must be finished off at a high temperature, speaking, of course, comparatively.

With regard to specifications, the writer maintained that the Author was here challenging the engineer. He also thought that the particular case he quoted was rather hard on the inspector, because a steel having an ultimate strength of 37 tons per sq. inch while having a combined elongation and contraction of 90 per cent. was undoubtedly the best material. He hoped that

(Mr. J. M. Lessells.)

decisions such as these would not reflect on the ability of the engineer what could not be said for the inspector concerned. Personally, most of the specifications with which the writer had had to deal brought out all that was good in the material. He did not think they had retrograded, at any rate because he considered the embodying of impact tests to be a very good step, since this test in skilful hands was a very good check on the heat treatment of the material.

In some cases the specifications left much to be desired. For instance, he would cite the case of a mild case-hardening steel which ought, in his opinion, to be accepted on chemical analysis alone, while carbon-steels ought to be prohibited in high tensile case-hardening specifications. While a carbon-steel might satisfy this higher specification, it lent itself very readily to bad treatment, while a nickel-steel was nearly "foolproof." Probably this was rather a question for the steel-maker, but it was worth noting. If any alterations were made to specifications, the engineer ought to see that the tests demanded conformed more closely than at present to the loads experienced in practice. The engineer would be pleased to hear of the optimism of Mr. Ashdown on the subject of "ghosts." He considered this question demanded the investigation alluded to by the Author.

Mr. C. H. RIDSDALE (Middlesbrough) wrote that it might be a satisfaction to Mr. Ashdown, and to those who were considering how far his recommendations could be relied on, to know that he (Mr. Ridsdale) could thoroughly substantiate many of these from his own experience. He had also always dealt with metallurgical principles in connexion with their application in practice in various Papers which he had given before kindred Institutions, and as these Papers afforded entirely independent corroboration on many points, he might perhaps give one or two references. Thus, as to the coarse structure of parts or forgings which had received little or no subsequent work (page 227), he himself had said,* "when a

* "The Correct Treatment of Steel," Journal, Iron and Steel Inst. 1901 vol. ii, page 81.

whole piece is heated and only part of it receives work, if the heating is gradual and lasts long (particularly if there is more than one heating, the piece not getting below low red each time), a large grain may be developed which is not broken up in the unworked parts." He had also referred to the same trouble in drop forgings,* and might point out that these from the high temperature at which they were forged were bound to have a very coarse grain unless cooled and reheated.

As regarded the bolts, heads of which had cracked off, he (the writer), referring to pieces which had been heated, but only a part of which had received work, had said,† "seeing that there is a weakness or internal strain in every forging it is desirable to remove this, and the most effective way is to let it cool and then reheat from the cold as rapidly as possible, consistent with the mass, to cherry red." This was about 900° C., and he might say he thought 677° C. was too low a temperature to which to reheat in order to "normalize" the structure effectually, hence the shelling off of the bolt heads. He was glad to see the Author had (page 229) so distinctly condemned the "misconception . . . . that full advantage can be taken of the initial forging heat . . . . such forgings must be allowed first to fall below its recalescence temperature, and then reheated above (this) and preferably cooled in air." He himself had condemned the practice.‡ "If it is of large mass, unless it has received plenty of work and been finished at fairly low temperature, say, low red (600° to 700° C.), don't put whilst still hot in a pit in the ground, or in annealing furnace, or even stock in large heaps, lest the very slow cooling may cause the grain to grow. This would do good to small sizes finished at low temperature (dull red or lower); but by far the best annealing is to let the steel get cold first and then reheat rapidly to cherry red (about 900° C.) for a short time; afterwards let cool again fairly rapidly."

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* "The Correct Treatment of Steel," Journal, Iron and Steel Inst. 1901 vol. ii, page 82.

† *Ibid*, page 81.

‡ *Ibid*, page 80.

(Mr. C. H. Ridsdale.)

With reference to the redevelopment of coarse structure when reasonable limits of temperature and time were exceeded, he (the writer) believed he had been one of the first to draw attention to these points, and had given a diagram * illustrating it. As one instance, that sort of fault sometimes occurred when masses of thick gauge sheets were annealed, and became entirely brittle or "rotten," so that they might break on dropping on to the floor.

He thought the plan the Author recommended (page 230) for heating forgings of widely different mass, first "to just the change-point," say about 704° C., was a very good one, provided the soaking was not too long; but it must not be forgotten that, as he (the writer) had repeatedly shown, within certain limits a long time at a low temperature—"soaking"—produced crystallization, as did high temperature for a short time. The Author very rightly condemned sand-annealing, except in a few special cases, and his remark on page 232, as to quenching forgings "only when uniformly heated above the change-point of the steel," was very cogent.

Again, as to "the quicker a forging can be cooled until the critical range has been passed, the better will be its structural condition," this was quite correct if interpreted with reasonable regard to the size and composition of the forging. For instance, he (the writer) had met with many instances of forgings which had excessively large grain due to slow cooling from a high temperature, and had always found that, if of soft steel and not too large, the grain could be best refined by rapid heating to 900° or 950° C., chilling from about 800° C., then again reheating rapidly to 850° C., and letting cool in air. There were other points which it would take too long to touch on, but altogether Mr. Ashdown was to be complimented on having given a very useful and practical Paper.

With regard to Sir William Beardmore's valuable Paper, he also, page 215, drew attention to the importance, whether steel was being kept hot or in process of cooling down, of *time* as a factor influencing grain size, and which necessarily took effect so much

* "The Correct Treatment of Steel," Journal, Iron and Steel Inst. 1901. Diagram. Vol. ii, page 73.

more when dealing with very large masses. He also referred to the very important relation between the amount of work put on a forging and the grain condition. This effect of work, of course, as the writer (Mr. Ridsdale) had stated,* only took place below a certain critical temperature, and below that, so far as his investigation had shown, it seemed to follow a fairly definite rule.†

The remark on page 222, "the best metallurgical conditions are incompatible with the best commercial conditions," was unfortunately only too true—at any rate, one continually met with cases where the metallurgical requirements for retaining the best qualities of steel were flagrantly violated from the desire for cheapness.

The method described for uniformly heating the large forgings, having parts of greatly differing diameters, was very interesting and instructive, and the results of mechanical tests showed a wonderful uniformity.

Sir William Beardmore wrote in reply that he cordially agreed with all that Sir Robert Hadfield had to say on the necessity and the value of pyrometers in steel works practice. Their uses were continually increasing, and as experience widened, the limits of temperature became closer to which works operations could be confined. As a consequence, a very much greater degree of uniformity was obtained in the finished work, and the control

* "The Correct Treatment of Steel," Journal of Iron and Steel Inst. 1901, vol. ii, page 66: "At this temperature (above critical point) the size and shape of the grain is not affected by work"; and page 67, "below this point—the grain becomes finer and more interlocked, and hence to a certain point the lower the temperature to which it is continued the more work increases toughness of material."

† "Practical Microscopic Analysis," Journal of Iron and Steel Inst. 1899, vol. ii. Table giving size of grain page 111, and page 114 diagram showing ditto in various sized sections. Also page 113, "in normal steel, for sections—finished below the critical temperature, but not below low red, under similar heat conditions (i.e. finished at the same pro rata temperature and subjected to the same cooling influences), in steel of the same composition the size of grain is *inversely proportional to the sectional area*, and when it is not so, this gives us an indication of the variation in other conditions."

(Sir William Beardmore.)

which the pyrometer gave enabled the works at the same time to undertake work to which they were unaccustomed and to undertake it with success. Sir Robert had mentioned manganese-steel and some of its peculiarities, but at the Author's works at Parkhead they had found that, armed with a pyrometer, the working and treatment of manganese-steel presented few difficulties, even to those who had had no previous experience of this valuable alloy. In the melting furnace the pyrometer had been found of use, though the differences which he had experienced were a little greater than the 30° C. between maximum and minimum values quoted by Sir Robert—the difference between highest and lowest temperatures on tapping being about 80° C.

Although the plan recommended by Dr. Rosenhain (page 243) for treating large forgings of varying sections would no doubt be as successful in practice as it seemed to be in theory from a purely metallurgical standpoint, he would remind Dr. Rosenhain that time was a very valuable element and costs in commercial life not unimportant, and his alternative method would certainly occupy a much greater time in the furnace and consequently be much more costly. At the same time the central parts of the heavy sections would be a longer time at their higher temperature, and it seemed therefore even from a metallurgical standpoint not to be so commendable as the method which he (Sir William) had described, and which had been used extensively in the works under his control.

He agreed with the importance of the effect of annealing between 600° C. and 700° C. on certain steels low in manganese, but he did not think that the note of warning sounded by Dr. Rosenhain would be seriously considered by those who were dealing with ordinary steels containing between 0·7 to 1·0 per cent. of manganese. The effect of manganese greatly retarded the balling up of the cementite, and he had never found any brittling effect to result from this treatment, while thousands of tons of steel of the correct composition had been so annealed.

In reply to Dr. Hatfield (page 247), the figures given on page 223 were obtained from the actual job, and he had no doubt

that, having once been obtained, they could be repeated without difficulty. In stating the relation between the cross-section of the ingot and that of the finished job, he did not of course mean to imply that that ratio of 3 to 1 gave the very best results that could be obtained, because every one knew that the more work a forging received the better—provided that correct technique regarding the heating went along with it. But his intention was to point out that for large forgings, unless such a ratio was attained, there was a likelihood that the structure of the original ingot would be found in the forging, and this structure could not always be removed afterwards by heat treatment from large masses of steel. This ratio was of course an empirical ratio founded on the results of works practice with the larger forgings, and it did not necessarily apply to smaller sizes. There were even large forgings which could not be made without less work being put on certain sections than was indicated by this cross-section ratio, and although in such cases compromise was necessary, in his opinion it was always advisable to get this minimum amount of work whenever possible.

Dr. Carpenter (page 251) had brought up the same point, and he (Sir William) hoped he had removed the misconception which seemed to exist that the words used in his Paper had all the certainty of a definite law, when they were intended merely as a useful guide. The figures which Dr. Carpenter had given in this connexion were very interesting, even though they applied to an alloy other than steel.

Mr. Remington (page 259) was correct in assuming that the definition of a large forging was one over or about 5 tons in weight, and the troubles likely to be encountered in such cases were greatly intensified, in the Author's opinion, as the size increased.

In reply to Mr. Somers, he (Sir William) agreed that disks manufactured in the manner described in his Paper were not so satisfactory as when manufactured so that "the strength circumferentially would be as uniform as possible round the whole diameter."

In answer to Mr. Whitfield's inquiry (page 265), the ingot for disks was forged down into a slab 24 inches thick from a temperature

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of 1,130°–1,140° C., and it was then rolled down to its final thickness from a temperature of 1,100°–1,110° C., while the larger ingot was forged starting from a temperature of 1,150–1,170° C. He did not think that Mr. Whitfield had dealt with the heat treatment of such large forgings, as illustrated in the Author's Paper, or he would have found the beneficial effects which oil-quenching had on plain carbon-steels. From his (Sir William's) experience he could state that it would not have been possible to get the test results wanted without oil-quenching, and Mr. Chew's views entirely confirmed his own.

Mr. Ashdown's remarks (page 275) in his opening paragraph were dominated by an accuracy more pedantic than real, for he did not think there would be any confusion in the minds of those to whom the Paper was of interest. It was not a subject that would appeal to those ignorant of the elementary principles of heat treatment. Nor were Mr. Ashdown's uses of the terms "annealing" and "normalizing" entirely free from criticism, for in his own Paper he had reference (page 226) to "annealing temperatures . . . . between 650° C. and 927°," and further on (page 228) to forgings being "normalized at 677° C." He agreed with Mr. Ashdown that terms so used tended to become vague.

As for the remark that the "bad" results from the middle of the first disk might be due to the forgings being treated lying on the bottom of the furnace bogie, the disks were not treated in this manner, so that it was probable that the explanation given in the Paper was after all more correct. The results of tests for the rotor-spindle were not the averages of a number of such forgings, but were taken at the same time from the one job.

He (Sir William) thanked Dr. Barr (page 277) for his most suggestive remarks on the form of test-pieces, and would state that when not otherwise mentioned the test-pieces were made according to the dimensions given by the Engineering Standards Committee. Every works had as the result of their own experience much accumulated data regarding the effect of composition and heat treatment on forgings of different sizes, and from these were deduced the information required for any new work. At the same

time such records were apt to be disconnected, and to leave gaps in our knowledge which could only be bridged by the judgment and skill of those in charge. And although experimental work could only be undertaken at great cost, there were many occasions when the cost of such experiments was gladly borne because it was justified by results.

Mr. H. H. Ashdown, before replying to the many interesting points and questions raised both at the Birmingham Meeting and those contributed by correspondence, desired to refer to points raised by the speakers at the London Meeting.

In further reply to Dr. Walter Rosenhain, the Author considered that the subject of the choice of quenching mediums was deserving of deep and careful consideration when dealing with heavy masses, more particularly with hollow work such as gun-forgings. He made a special point in inferring that modifications in the analysis of forgings intended for water-quenching should receive careful consideration, and he would invite Dr. Rosenhain's attention to the very able Paper presented by General L. Cubillo to the Iron and Steel Institute, and published in Vol. 1, 1912. It might not be generally known that many thousands of forgings of special alloy steels, and of a most complex character and form, were water-quenched with surprisingly good results in this country, and although Dr. Rosenhain's condemnation of the employment of water as a quenching medium for some forgings might be deserving of attention, this matter should not be swept aside without receiving more serious consideration.

The method of annealing large turbine-shafts, as suggested by Dr. Rosenhain, did not altogether commend itself to the Author, as in the first instance the shaft-ends admittedly were badly overheated, and for refining they were dependent on most careful attention to both the temperature of the body of the shaft and the cooling of the shaft-ends below the change-point, which must be followed by a recovery of the heat above the critical range. Further, in addition to occupying a relatively large amount of furnace-room, each shaft must be dealt with individually.

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In reference to Dr. Hatfield's remarks, in the first instance the Author was not in agreement with him that he, the Author, had hardly done justice to steel-makers in general and to himself in particular. The Author was not before the Meeting to uphold all that was best, but to call attention to irregularities, and this he did for the benefit of the steel-maker and engineer and also in diffidence to himself, as he considered by calling attention to such points as those brought out in the Paper it had far-reaching and beneficial effects. He quite agreed with Dr. Hatfield that the large steel works in the country were well equipped and fitted up with the best scientific instruments for heat control, but he, the Author, questioned if anything like the fullest advantage was taken of them and if the results obtained were all that could be desired.

With regard to Dr. Hatfield's remarks concerning the application of science to industry, the Author's contention was that the great value of research was often lost to the works owing to its want of application. This apparently had not only been the Author's experience, as it might be noted that, recently before the Faraday Society, Dr. Armstrong stated: "The chemist of the research type would have difficulty in passing from the laboratory to the commercial scale his discoveries and in conveying his requirements to an engineer." It was not every young man who passed through the research laboratory who could be placed in responsible positions in the works; such selections should be made with great care. He (Mr. Ashdown) had seen well-trained and highly qualified young men prove themselves hopeless failures in the works.

The Author, before replying to the discussion on his Paper at the Birmingham Meeting, desired to preface his remarks with his appreciation of the interest the Birmingham members had shown in the subject of heat treatment, and much regretted, owing to the heavy demands on his time, his inability to be present to meet the discussion in person.

Mr. A. A. Remington (page 259) was quite correct in assuming that a forging weighing 5 tons might be considered as the low limit of heavy forgings, and although forgings weighing as much as 60 tons were at present made, he, the Author, did not think the

high-water mark had yet been reached. The Author, although having in mind forgings of the heavier type, had very briefly referred to forgings of all descriptions even to drop stampings, as his experience had been of a general character, and further, although not specifying the weights, he thought the forgings mainly embodied in the Paper, in themselves, conveyed a good idea of their relative weights.

With regard to excessive heating, the Author had stated that he had never found it necessary to exceed a temperature of 843° C. (1,550° F.), and questioned the desirability for working at so high a temperature as 927° C. (1,700° F.). Attention had also been called to the most important time factor for heating; but this could not be specified, as the duration of heat must depend entirely on both the size and quantity of forgings at any one time under treatment.

Mr. Remington referred to the Author's statement that makers of large forgings experienced their troubles; this was admitted, but the Author could honestly say they were small in proportion to the great volume and huge masses which they were called upon to produce.

It had been the Author's privilege to visit within recent years several of the most up-to-date works in Birmingham and district, and he was satisfied that, although many excellent heat-controlling systems had been installed, many failures yet occurred owing to the lack of observation or the application of elementary scientific principles. The Author was pleased to see the prominence which had been given to the British Standard Specifications for Wrought Steels for Automobiles, and a wider circulation together with more careful attention to the advice given should be of great service to all concerned in the treatment of steel. Mr. Remington would appreciate there was a wide difference in knowing how certain things should be done and putting the same successfully into practice. It was surprising how many fell short in the latter in the most essential details. With regard to quenching mediums, the Author would refer Mr. Remington to his reply to Dr. Rosenhain on this same subject.

The Author quite agreed with Mr. Remington that great

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advances had been made in recent years by the application of science to industry, but greater strides might yet be made along the lines the Author had indicated. When the War revealed its secrets he was not sure that Mr. Remington would be right in his statement that the greatest advances had been made with small mass production. Regarding specifications, to the Author's mind, reasonably framed specifications in the hands of competent inspectors would reduce waste, reduce cost, increase production, and place us in a healthy position to meet perhaps the keenest competition with which this nation had yet had to contend.

The Author welcomed Mr. Somers' contribution, and, although he had not had the pleasure of visiting his works, he had heard much of them and their up-to-date methods through his friends while in the service of Messrs. Thomas Firth and Sons, Sheffield. The Author had only given as an example the possibility of the necessity of two heats on a marine shaft, and he thought that Mr. Somers would agree that, with forgings weighing from from 70 to 80 tons, several heats were necessary, wherein the structural conditions instanced in the Paper would hold good. Again, assuming it was possible to forge out in one heat double intermediate shafting 45 feet in length, Mr. Somers would appreciate that the structural condition between the two ends must be widely different, owing to the great difference in the finishing temperatures.

Referring to the subject of "ghosts," the size and quantity of the ghost-marked areas had to be considered. The Author had seen and had in his possession photographs of great ingots which might be looked upon as masses of ghosts retained in position by the surrounding steel. For the majority of purposes to which heavy forgings were put, ghost-marked steel was neither detrimental nor objectionable, but there were other purposes, and on those the Author proposed at present to remain silent, where ghost-marks were not to be desired. He, however, confirmed that with care it was possible to make heavy ingots practically free from ghosts.

In reply to Mr. Whitfield, the Author would point out that the

subject under review was one on which much could be written, and the greatest difficulty the Author experienced in preparing the Paper was conciseness in covering so wide an area. With regard to the heat treatment of steels, it would appear that Mr. Whitfield in the first instance inferred that excessive temperatures were necessary, such that would reproduce a casting structure (the object for which the Author failed to follow); it then, of course, would be imperative to follow up with a high temperature annealing in order to refine the casting structure so produced. It was not the intention of the Author to be severe upon the steel manufacturers by conferring upon them much of the responsibility for the so-called fatigue. It was his desire only to call general attention to the cause of the coarseness of the grain to which Mr. Whitfield referred, and to point out how this could be remedied. With regard to "ghosts," the Author did not agree with Mr. Whitfield's proposed remedy; he, the Author, was opposed to promoting segregation, and strongly favoured the more rapid solidification and prevention of segregation.

Mr. Chew, in referring to sand annealing, had, the Author thought, misunderstood his statements, and he thought a more careful perusal of that paragraph would be helpful to the speaker and probably save him much time and inconvenience by, as at present, having to seal up his furnaces when annealing forgings. When inviting members to explain the reason of high temperature quenching, the Author wished to infer that temperatures were employed by some which exceeded the upper critical range by several hundred degrees. The Author thought that Mr. Chew on inquiry would find that the temperature of 840° C. well exceeded that demanded by the steels under consideration. If Mr. Chew ascertained what the upper critical range was of the steels as covered by the Paper, including those of nickel chrome, he would find the temperature mentioned by the Author considerably exceeded that necessary to effect a maximum hardness on quenching. The Author agreed that for very low carbon-steels or for tool-steels the employment of higher temperatures was necessary, but these were not relevant to the Paper.

The Author regretted that Mr. J. A. Hannay had met with

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disappointment in the Papers presented, and he, the Author, agreed that such Papers were not presented to please, but to reveal the weak spots, which unfortunately did cause a little irritation to some. If Mr. Hannay would refer to Fig. 6, Plate 4, and note the difference in the relative heights of the forgings in the furnace together with the positions of the pyrometers, with a little reasoning he would appreciate it was possible to obtain a difference of at least 300° F. By his contention, assuming he had but one pyrometer connected up to a recorder, he would be satisfied that his furnace-charge was in accord with his record—but what of the results? The Author maintained that recorders and pyrometers were more misleading than helpful unless they were used with reasonable discretion.

The Author was not aware that he had given the impression that there was any guesswork in connexion with heat treatment of heavy forgings; on reference to Fig. 16, Plate 8, Mr. Hannay would see that methods were adopted for checking the time of heat penetration through heavy forgings on varying levels in the furnace. He was also under an entirely wrong impression if he assumed that there was little to be learned from makers of heavy forgings. The colossal number of forgings supplied during this present strife and the marvellous results obtained were in themselves a testimony to the steel-manufacturer.

Lieut. W. R. Parsonage questioned the use of the Siemens Water Pyrometer. The Author was pleased to confirm that it had been of great service in the past, and in skilled hands it could be used with surprising accuracy. Like the Brearley Sentinel, the Siemens Water Ball could be placed in any position in a furnace, and any distance down hollow forgings in "pockets" in steel castings and places quite inaccessible to the stem pyrometer, thus enabling temperatures to be read from any desired point. Further, their use for checking the heats of furnaces containing large charges for either annealing or hardening gave sufficiently accurate results, and their constant insertion and withdrawal from the furnace enforced the operator to keep his furnace-charge under closer observation.

The Author much appreciated Dr. Andrew's contribution (page 272) to the discussion, and in reply he was pleased to state that the types of steel dealt with in his Paper were as Dr. Andrew assumed, namely, carbon, nickel and nickel-chrome steels, such as were in general use now for ordnance, marine and like purposes. It would appear to the Author that Dr. Andrew was referring to the recalescence range, wherein the Author agreed those temperatures might be well below that of 650° C. (1,200° F.), but in order to produce this, one must first exceed considerably the temperature of 650° C. If Dr. Andrew would again peruse the paragraph in question, he would see that the Author distinctly referred to the calescence temperatures under which little change was effected in the structural conditions of steels, and that the statement made by the Author held good.

With regard to the term "normalizing," the Author had made use of it in the same sense as it had been, and still was, used in many works, and he had himself called attention by a foot-note on page 228 to the correct interpretation of this term in the same specification to which Dr. Andrew referred. The Author quite agreed that for scientific exactness he should have referred to the physical change taking place during the cooling period as critical ranges, but he thought this was general knowledge to all who had made any serious study of the subject.

The Author was at some loss to follow Dr. Andrew's point regarding the use of pyrometers; he was not aware that he had cited any case against their use. In all the works in which he had been interested, he had everywhere encouraged and developed their use. The point the Author had endeavoured to bring out, and one which he would most strongly urge as the result of years' experience, was not to accept the readings shown on a record without knowing that careful attention had been paid to the material under treatment by a careful and reliable furnace-operator. Dr. Andrew did not appear to have considered these points in their entirety as stated by the Author. He was in full agreement with Dr. Andrew concerning the points brought out relative to the application of research work in the works. He hoped that this

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matter would continue to receive the careful consideration and attention of all who were interested in the progress of our great steel industries.

The Author much respected Dr. Archibald Barr's appreciation of his contribution, and if it had further encouraged the study of this subject the Author felt that a little more progress had been made. With regard to mechanical testing, he quite appreciated the points brought out by Dr. Barr, but the only reason he had submitted the results on Plate 4 was to show the effect of the structural condition on the mechanical properties, and in this instance the tests were strictly comparative. In replying to Dr. Barr's question relative to the 30-ton forging under treatment, the particular forging from which the microsections were cut was a hollow forging with a 10-inch wall, and in each case the microsections were cut from the centre portion of the wall, that is, 5 inches from the outside. He quite agreed with Dr. Barr's expressions relative to the infusion of science into works practice, and in the Author's opinion it was a matter deserving the fullest consideration of all works management.

In replying to Mr. J. Victor Harrison's criticism (page 280), relative to the use of pyrometers, he would suggest to the writer that any subject was open to grave misrepresentation and misconstruction if it was not treated as a whole. The Author was not aware that he had anywhere in the Paper suggested such a retrograde step as to abandon the use of pyrometers and to depend on the observed heat by the furnace operator, especially for temperatures so low as 500° C. Such a practice the Author would never condone. However good a judge of heat a furnace-hand of long experience might be, he was apt to considerable error owing to the constantly fluctuating conditions of external light. The Author would commend to Mr. Harrison for careful thought the following sentence, which appeared in the Paper under the heading of Pyrometers (page 230):—"When it is fully realized that pyrometers simply indicate a local temperature, and that uniformity of temperature (particularly when dealing with large quantities of small forgings, or masses) is entirely dependent on careful

observation, many of the so-called mysteries, together with their consequent troubles, will disappear."

To those members whose experience might have been somewhat limited in the use of pyrometers, the Author would suggest the advisability of both a spare "couple" and an "indicator" always being kept as a standard, as it was no uncommon experience for one to find "couples" (pyrometers) reading incorrectly by as much as 50° F. Possibly an exceptional case of this description came before the Author's notice within recent date. An "indicator" was found to be reading incorrectly by 200° F. and was returned to the makers for readjustment; after a lapse of several weeks this instrument was sent back to the works as correct, but on checking against a standard the error was still found to be the same. The makers admitted this was an unfortunate oversight, and the Author merely mentioned this as one of many possible sources of error. A very cheap, effective, and efficient "standard check" was the Brearley Sentinel Pyrometer, which should be placed adjacent to the end of the pyrometer and the furnace heated up very gradually; the fusion of the "sentinel" denoted the temperature stamped on the sentinel wrapper. From long experience the Author had found this an absolute check.

Judging from the results of tests obtained by Mr. Harrison from some of his forgings, although they might have passed test, it would appear to the Author that they required a treatment other than merely heating to 500° C. in order to ensure their being in a reliable condition when under stress.

Mr. J. M. Lessels raised several points deserving of both attention and consideration. The subject of drop stamping was one on which much could be written, and the development of this industry was highly commendable to both the smith and the engineer. As so many different types of steels were conformable to hot stamping, the matter of both the heating and finishing temperatures was open to individual consideration, and this again depended on the size and shaping of the piece before final stamping. It was, however, regrettable to note at times the ill-treatment to which valuable steels were subjected. They were heated

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both unnecessarily long, and to a temperature bordering on fusion, and when occasional stampings were fractured for examination they showed both a slipping and a rupture of the crystals. The subject, as dealt with by the Author in the Paper, was the final heat treatment, and in his opinion no stamping should be allowed to leave the works without having been subjected to at least a "normalizing" treatment as defined by the British Standards Specification for Wrought Steels for Automobiles.

With reference to specifications, the Author in his opening remarks stated "that they leave much to be desired," and he personally considered that whilst such specifications still existed the experience of the inspecting engineer should be such that he could use discretionary power in accepting materials which he knew to be more than adequate to the purpose for which it was required.

The Author was not in full agreement with Mr. Lessels in his assumption that nickel-steel was, as he termed, "foolproof," as compared with carbon-steel. Any steel which was subjected to a prolonged heating at a comparatively high temperature, such as was called for in the process of case-hardening, was liable to develop a coarsened structure, and in order to obtain the best results, whether a carbon or nickel-steel were employed, a refining heat treatment should be given.

The presence of "ghosts," as was generally known, was due to the enclosures of non-metallic impurities, and both the size and quantities of ghosts depended on the quantity of these enclosures. In a Paper by Dr. Arnold * it was stated that ghosts in steel would not be got rid of in our time. By this it was inferred that ghosts were sometimes formed in the major portion and serviceable part of the ingot. The Author was unable to agree with this statement, as he had seen many forgings made from huge ingots which after machining and polishing had been absolutely free from ghost-marks. Again, it was better to admit that the presence of ghosts was not desirable, and although some evidence had been

* Proceedings, I.Mech.E., 1915, page 653.

submitted which would suggest as the result of certain tests their presence was an advantage, other tests and investigations would lead one to think otherwise. Although admitting in the first instance their presence was objectionable, it by no means condemned the great majority of forgings revealing these marks, and it was here where the intelligent engineer was expected to use reasonable discretion. Perhaps forgings, such as torpedo air-vessels, owing to their exceedingly light section, the great pressure and the important functions expected of them, might be viewed with some suspicion when these marks were plainly visible. Yet the Author had known of a number of such forgings so marked having been tested to destruction, and in no case known to him had the ruptures occurred in what had been considered the weak areas. Again, in gun-forgings bad ghost-marks, whose presence might be viewed with a certain amount of suspicion in inner tubes, should be considered from an entirely different standpoint for the other forgings assembled into the gun, and should be looked upon with much less importance for the external tubes. The Author had known of huge jacket-forgings being rejected for this cause, and yet these jacket-forgings were considered more in the light of a balance-weight than adding strength to the gun. The presence of ghosts—enclosures in shafting and turbine-machine parts—as already stated, was not desirable, as they formed centres from which corrosion commenced. This, however, should be no detriment when one remembered the masses of enclosures contained in the best wrought-iron made, much of which had given remarkably good records during the long periods they had been in use.

The Author greatly appreciated Mr. C. H. Ridsdale's contribution, in so much that the main points brought out in the Paper were confirmed by Mr. Ridsdale's own experience. The latter gentleman's experience in both laboratory and works practice had been very extensive, and his statements might be regarded as deserving of the closest attention. The Author was aware that various Papers referring to this subject had been presented before other large technical societies, but in the preparation of this Paper the Author had not had recourse to any such publications. He

(Mr. H. H. Ashdown.)

really looked upon his Paper, in the main, as a résumé, or as what Dr. Stead had recently labelled a scientific stocktaking.

With regard to the heating of stampings, indeed any forgings, to a temperature of 677° C. as a final heat treatment, the Author agreed that this was much too low. This was a temperature to which such forgings had been heated, and they subsequently failed under shock test. In the following paragraph the Author stated (page 228): "Now after these forgings received a proper heat treatment (which was 870° C.), a uniformity in structure was obtained together with the disappearance of any further shelling of the head when subjected to the same repeated shock stresses." If, as was suggested by Mr. Ridsdale, the Paper was of practical value to the members, the Author felt gratified in having accomplished the main object he had in view when presenting a Paper on the subject of Heat Treatment.

The Institution of Mechanical Engineers.

PROCEEDINGS.

April 1917.

An Ordinary General Meeting was held at The Institution of Civil Engineers, London, on Friday, 20th April 1917, at Six o'clock p.m.; Michael Longridge, Esq., *President*, in the Chair.

The Minutes of the previous Meeting was read and confirmed.

The President announced that, to fill the vacancy among the Members of Council caused by the election of Dr. Dugald Clerk as a Vice-President, the Council had appointed Mr. Richard Williamson a Member of Council. He would retire at the next Annual General Meeting, in accordance with Article 25.

The President announced that the following Transference had been made by the Council :—

Associate Member to Member.

Smith, Joseph David, ... Newcastle-on-Tyne.

The President then delivered his Inaugural Address.

The Meeting terminated at a Quarter past Seven o'clock. The attendance was 112 Members and 42 Visitors.

ADDRESS BY THE PRESIDENT,

MICHAEL LONGRIDGE, Esq.

It is a custom worthy of honour by observance that the President of this Institution should devote the first sentences of his Address to an attempt to express his appreciation of the great honour the Members have conferred upon him. It is not easy to find the right words for such a task, but if each one here will call to mind the names of our Past-Presidents engraved upon the marble panel in the Institution Hall, commencing with George Stephenson, who taught my father his profession of a civil engineer, and ending with William Cawthorne Unwin, whose great attainments we all recognize, and imagine how great would be his pride if he found his own name added to that roll, he will understand my feelings far better than I can express them. But pride of place and office is not the predominant element of my content. It is rather the knowledge that I have been accorded the confidence and esteem of those for whom, with whom, and even against whom, I have worked since I became a Member of the Institution thirty-seven years ago. It is for that I am most grateful. It is for that I thank you even more heartily than for having elected me your President.

I believe it has also been customary for our Presidents to base their Addresses more or less upon their own professional experiences, reviewing the more recent developments of those branches of Mechanical Engineering with which they may have been most intimately associated. Had we been living in normal times, I

should probably have followed a similar course, and held forth on the changes which have taken place during my own engineering life in power plants for driving works of various kinds, especially textile mills and factories. I think the developments and inventions in connexion with these during the last fifty years would have provided excellent material for a President's Address.

But in the circumstances of to-day I feel that a review of this kind would excite less interest than impatience. Its scope would be too narrow. Our minds are occupied with matters of far greater moment than the developments of power plants for driving mills and works. They are fixed upon the War, and the evolution that must follow it. The War has taught us many things, but none more clearly, more insistently, than the importance of our Engineering Industry. Upon this industry we depend for victory to-day and for security and prosperity to-morrow. Should it be wrested from us, as many industries have been during the past half-century, England would fall to the rank of a second-rate Power, and the British Empire would be dismembered. The War has made us realize the possibility of this great disaster, and has brought home to us the need of action to avert it. And the need is urgent. The victory we all pray for may be won before my successor takes my place. Then we shall be plunged at once into a commercial struggle, and, unless we adopt my old school motto, "sicut serpentes sicut columbæ"—"wise as serpents and gentle as doves"—into an industrial war also, both fiercer than any we have hitherto had to face.

America is rich beyond imagination, and is systematically preparing to capture the world's markets if she can. Germany, if reports be true, is organizing her industrial resources in a way that we, with our strongly individualistic proclivities, cannot hope to rival. Neutral nations are growing rich at the expense of those who are fighting for their future independence. It therefore behoves us to take stock of our position and plan measures, not merely to hold our own, but to regain the pre-eminence which British manufacturers have lost. Why was that pre-eminence lost, and how can we regain it? These are the questions which interest

Mechanical Engineers beyond all other questions at the present time. And so I am tempted to speak about them, when, perhaps, silence would better become my inexperience, for I am neither a merchant nor a manufacturer. I shall ask you, therefore, to look back for a moment on the past, to view it from the standpoint of to-day—the retrospect may give some clue to the causes of our failures—and then I want to mention some of the things which we have done to repair our losses and some of the many we have yet to do. I do not wish to dogmatize, but rather to suggest.

When our Institution was founded in 1847, Mechanical Engineering, together with the other industries it served, was rapidly acquiring that pre-eminence which was soon to earn for England the title of the Workshop of the World. The Mechanical Engineer was favoured by circumstances of time and place. The country's natural wealth in coal and iron-stone had supplied him with the raw material of his trade. The improvement of the steam-engine by James Watt had given him power to drive his works. The evolution of the locomotive and the railway by George and Robert Stephenson had provided cheaper and more rapid means of internal communication than were available elsewhere. The application of steam-power to navigation initiated earlier in the century by Robert Fulton and Henry Bell, the numerous harbours on our coast, and the protection of a Navy supreme upon the seas, facilitated and safeguarded trade abroad. Finally, the abolition of the corn laws, if it devastated agriculture and hazarded the safety of the nation, undoubtedly assisted the Mechanical Engineer, first, by driving the rural population to the towns and thus providing labour for the workshop, and, secondly, by enabling foreigners to become his customers by paying for his productions with their corn. The extraordinary expansion of the cotton trade following the inventions of Hargreaves and his fellows, and the substitution of iron for wood in shipbuilding, also brought no small prosperity to the Mechanical Engineer. With such advantages, it is not surprising that before our Institution came of age England had won the title she has now for a long time lost.

In winning this title she was transformed from an agricultural and practically self-supporting nation into a manufacturing community dependent for its existence, as we realize too well to-day, upon imported food.*

And as the Mechanical Engineer was responsible in no small measure for the transformation, so he must be held responsible for the maintenance and efficiency of the workshop on which the feeding of the people and the defence of the people against their enemies now depend. He became and he remains a Trustee for the British Empire. How did he discharge the trust? By humbly seeking knowledge to turn the gifts of Nature to the use of man? By invoking the aid of science to develop the discoveries of the men who had prepared the road to his success? By caring for the welfare of the thousands who were spending their waking hours in his factories? By giving them a fair share of the profits of his business? I think we have the grace to-day to answer NO. I think we are willing to confess that our heads were turned by elation at our prosperity, that we were obsessed by admiration of our own achievements; too confident of the sufficiency of our limited knowledge; too contemptuous of the few who tried to throw the light of science on our path; too eager for wealth, and the social influence wealth could buy in the new state of society; too careless of the needs and aspirations of the "hands" who helped to make the rapid accumulation of large fortunes possible.

And what has been the consequence? For every lapse from the ideal, and there is an ideal even of industrial polity, Nemesis Adrasteia sooner or later enacts retribution.

Let me unfold the story of the engine-building trade of Lancashire. There was a time when shops were many and full, when Lancashire steam-engines were sent wherever steam-engines were used, when Lancashire's supremacy in this branch of

* Wheat consumption of the United Kingdom:—

Year	Home grown.	Imported.
1842	22,000,000 quarters.	2,970,000 quarters.
1914	7,300,000 ,,	29,220,000 ,,

Engineering was beyond dispute. In 1900 I went to the Paris Exhibition. I went there primarily to see the stationary engines, whose claims in the matter of steam consumption were beginning to attract attention here. What I saw there "gave me," to use a French expression, "furiously to think." I received a revelation. In elegance of form, in completeness of finish, in careful arrangement of details, the engines exhibited by some of the Continental makers excelled any I had seen before. The British stationary engines, small in size and number, slovenly in finish, makeshift in the fitting of accessories, such as cylinder clothing, lubricating apparatus, wheel-guards, savealls and the like, and sadly lacking in artistic inspiration of design, proclaimed to seeing eyes the dangerous excellence, not to say superiority, of the foreign work.

Ominous as the Exhibition was, it failed, so far as I could see, to produce any important changes in the practice of British engine-building firms. Nor is this very surprising. Those in authority in the drawing offices and works were still men trained exclusively in the shops. The "product of the Technical School" was still widely held to be a useless, if not a noxious product, and perhaps in the early days of technical education his training did not make for easy correspondence with his new environment. At all events, he had not yet acquired any effective influence in current engineering practice or design. The consequence was that when, as time went on, repeated loss of orders forced British Engineers to recognize the estimation in which Continental work was held by customers, royalties were paid for foreign patents, foreign draughtsmen were employed, and foreign drawings bought. And, later still, to fill up the cup of our humiliation, complete steam-engines built abroad were brought to England, erected and set to work by foreign workmen, not only for public bodies, which thereby justified the gibe about their lack of souls, but in the very home of British steam-engine manufacture, in Lancashire itself.

In the meantime British engineering firms, whose names were household words, were being wound-up, or ceasing to be dividend-paying concerns,

In some cases the inducement to buy abroad was lower price, in some earlier delivery, in some convenience of collaboration with makers of electrical machinery; but perhaps in most it was a guaranteed rate of steam consumption which could not be obtained at home. In either case we ought to have been able to offer the inducement here. It is entirely our own fault that we could not.

In the nineteenth century few British engineers were able to guarantee the steam consumption of their own steam-engines. To-day most contracts contain consumption guarantees. If these guarantees are not so low as those obtainable abroad, I think their fulfilment sometimes falls less short of promise.

I have taken my illustration from this particular branch of engineering, not because it was especially conservative, but because many of those interested in it are my friends and know that I speak from conviction, not from malice. I believe that most other branches of Mechanical Engineering have suffered more or less from foreign or American competition. Some of the causes of our relative retrogression are beyond the control of Engineers, others they can remove in part or altogether, and of these I think inefficient technical education, lack of trade organization, and the policy of the Trade Unions, claim special attention.

Technical Education.

In the early days of our Institution most engineers were trained entirely in the shops. Theoretical knowledge was uncalled for and even held to be antagonistic to practical success. There was no supply of scientific engineers, partly because there was no demand, and partly because the places where a man could study the theoretical side of his profession were few and far between. It was only in 1840 that the Chair of Engineering, subsequently filled by Rankin, was established at Glasgow University. There were engineering courses in London at King's College, and courses dealing more particularly with mining and shipbuilding at the School of Mines and at the School of Naval Architecture. In the provinces there were a few technical colleges, such as the Merchant Venturers' Technical College at Bristol and the Owens College at

Manchester, though it was only in 1868 that the first Professor of Engineering, Osborne Reynolds, was appointed there. In the great centres of the iron and engineering trades opportunities for technical education, beyond what might be acquired in Mechanics' Institutes, were practically non-existent.

It was not till 1877 that the Livery Companies of London, belying the popular idea of their activities, appointed a Committee to consider the possibility of initiating a National scheme of Technical Education. The City and Guilds Central College was the result. Its foundation in 1880 marked the first public recognition of the need of something more than office or workshop training for the Engineer. Then came the appointment of the Royal Commission, which reported in 1884, the formation of the National Association for the Promotion of Technical Education in 1887, and the Technical Education Act of 1889, which refused to the schools it governed the means of teaching the practice of any trade. In 1890 only was a real foundation laid by the transfer of the control of technical education from the School Boards to the Local Authorities, and the appropriation of half the "whisky money" for its support.

The result has been a vast expenditure on Technical Schools. Nearly every Local Authority has built one. These schools range from the technical school of the small borough, whose engineering instruction is limited to evening classes in "Engineering drawing, Practical mathematics, and (I quote from the prospectus of one of them), Engineering science," whatever that may be—they range from such schools to institutions of University rank like the Municipal School of Technology in Manchester, with its numerous laboratories, and among them engineering laboratories and workshops equipped with full size modern machinery, tools and apparatus, not only of the types in general use, but also others specially designed for demonstration and experiment, where the science of engineering and the outlines of its practice can be learnt, and where research is carried on by professors of high attainments and post-graduate students in connexion with the workshops of the district.

Besides these we have provided engineering courses in most of our older Universities, and endowed new, where science enjoys more prestige than Greek. To Oxford, Cambridge, St. Andrews, Glasgow, Aberdeen, Edinburgh, Dublin, Durham, London, we have added since the foundation of this Institution, Manchester, Wales, Birmingham, Liverpool, Leeds, Sheffield, Bristol, Belfast, and the National University of Ireland.

With all these schools and colleges it might be thought that the problem of educating engineers and engineering workmen had been completely solved. Unfortunately it has not. Our innate conservatism ties us too much to the traditions of the past. Many still fail to understand that the manual training which enabled an apprentice to become a master craftsman in times gone by, does not suffice to turn a schoolboy into an engineer to-day. The functions of the engineer and craftsman are entirely different, and their training must be different also. Moreover, differentiation is needed in the training of the various classes of engineers and workmen. Designers and scientific advisers require a knowledge and experience which managers and business organisers can forego without much disadvantage. Foremen and supervisors similarly need a wider range of practical experience and technical knowledge than men destined to be manual workers all their lives. This lack of differentiation seems to be one cause of the inefficiency of our Technical Education relatively to its cost. Another certainly is insufficient preparation of those entering the Technical Schools.

On the whole the education available for the higher ranks seems fairly satisfactory; at all events, it has produced men capable of solving many of the new and difficult technical problems presented by the War. Yet, on behalf of these, I would give one caution and make one complaint. There are many Englishmen of the best type with initiative, ability to organize, and power to lead and manage men, yet really incapable of assimilating much book learning, especially higher mathematics. Do not let us make the mistake of forcing these men through the same collegiate course as men of different mentality, and then passing them on examination

with a low standard of full marks. It would be infinitely better for everybody to keep them to more elementary work and require a high standard in examination. We must not underrate these men in our new-born zeal for scientific formulæ. Many of them are of our very best. The Institution recognizes their value by admitting them without examination at 30 years of age. I hope the rule will not be altered. It is astonishing how much sound engineering can be done with the aid of $s = vt$, $v^2 = 2fs$, $P = mf$, etc., and corresponding elementary relations in other branches of natural philosophy, if only their meaning be well rubbed in, and their application clearly understood.

And my complaint. It is against the obstinacy of our two most famous Universities in retaining Greek as a compulsory subject in their examinations.* This reacts upon our Public Schools, and is a serious handicap on those who, intending to deal with the concrete rather than the abstract in their future lives, yet wish to find their level in the social life and moral discipline of these two Universities. The English Public School boy can generally be relied on to face difficulties, lead men, and keep his hands clean in business. Engineering cannot afford to lose him to satisfy the country parsons who rule Oxford and Cambridge in this matter.

The education provided for the workmen, on the other hand, both general and technical, is most unsatisfactory. Trade apprentices who enter the shops at 14 years of age, and sometimes earlier, seldom have subsequent opportunities for learning other than at evening classes. Some are resolute enough to attempt brain work at the end of a day beginning between 5 and 6 A.M., and some are strong enough to profit by their attempt. The majority

* There is some hope that compulsory Greek will be abolished. Addressing a recent Conference of the Educational Association no less a personage than the Master of Balliol is reported to have said: "The movement to take in natural science was not hostile to the older subjects. He was quite sure that the opinion in his College and widely diffused through the University was that there was room for the two, and that a great deal of the old curricula might with advantage be dropped to allow new inspiring subjects to come into University education."

are not. I have seen in some paper that technical education in England and Wales is provided in 6,876 evening and similar schools, but that the average attendance is about one hour per week. Whether the average for engineers is greater or less, I do not know.

In parts of Scotland continuation schools are provided in connexion with the industries of the districts, and parents and employers are compelled to send young people to them during working hours between the ages of 14 and 17 years. In England, the difficulty is overcome in some large works by allowing promising boys to spend some of their working hours at technical schools without loss of time or pay. In others, technical instruction is given in the works. Unfortunately British engineering works are generally too small, and the managers too unwilling to co-operate with each other to allow such arrangements to become general. Yet the workman must have better education to qualify him to rise if capable, and to give those who have not the ability to rise some interests outside their daily work and football matches; and also to lessen drunkenness. The need will become greater as repetition work and automatic machinery replace varied jobs and manual skill. Unless an antidote be provided, the monotony of this kind of work will crush initiative and mental vigour, and instead of skilful workmen we shall breed incompetent machines. I have been told that there is more discontent among minders of automatic machinery than among men who have to use their brains and hands, and I can well believe it is so. Either the age for leaving school will have to be raised or some scheme devised for combining technical instruction (not mere craftsmanship) in the works, with general instruction in the schools in working hours—unless engineering employers refuse to take lads into the works till they are 15 or 16 years of age. Apprentices entering works from the junior technical schools at 15 or 16 will be better workmen at 21 than those entering at 12 or 14 from the elementary schools.

I think, also, that a clear distinction should be made between Technical Colleges of University rank on the one hand and Senior Technical Schools, whose entrance standard is lower than University matriculation, on the other. The interests of Engineers require

two classes of institutions giving technical instruction, each with its definite purpose :—

(1) Technical "Colleges."
(2) Technical "Schools" (senior and junior).

The Technical "Colleges" should be of University rank, and should be departments or faculties of Universities, like the Municipal School of Technology in Manchester, not independent of them, as in Germany. They should provide two courses, one for the scientific advisers and designers, the other for the managers and business organizers. They would receive their students mainly from the higher secondary schools. Admission should be by examination, or very strong recommendation by qualified persons, in the case of men past undergraduate age. The gate should be opened only to those capable of profiting by the teaching. The idea of University education for the million is most pernicious nonsense.

The reason for making these schools departments of the Universities is that the connexion would prevent overlapping of teaching, and would ensure that uniformity in examination standards which some of the Universities are trying to bring about. By uniformity of examination standards I do not mean uniformity of Technological teaching. In this respect some diversity, and some degree of specialization, would be desirable.

The Technical "Schools," both the senior, and the junior which are frequently housed with them, should also have two courses, one suitable for men likely to become foremen or supervisors, the other for those likely to remain manual workers. These courses should not be purely technical. The schools would receive their students from the elementary and lower secondary schools. They should remain under the local authorities, so that the technical teaching might be varied in accordance with the local trades.

Another desideratum is co-ordination of the work, especially between the higher Secondary Schools and what I have called the Technical Colleges, and between the elementary and lower Secondary Schools and what I have called the Technical Schools, so that pupils

passing from the one to the other would be properly prepared for the change. We also need a uniform system of adequate scholarships, to enable the best intellects among the poor to go up the whole educational ladder and mix on equal terms on each rung with those better endowed with this world's goods. The number and values of the scholarships available depend at present too much upon the ideas of the various Local Authorities.

Organization of Engineering.

And now I must pass on to my second point—organization, or, rather, lack of it. Mechanical Engineers are broadly divided into two classes—manufacturers and consultants. The latter, I believe, consider themselves the superior class, because they have been able to impose their wills and specification on the manufacturers. They can do so still to some extent. In the future they will do so not at all.

There was a time when the Consulting Mechanical Engineer sent out his own drawings, and the manufacturer had to work to them. Now the Engineer sends out his specification, but the manufacturer makes his own drawings and works to them, or offers some standard article complying more or less with the specification of the Engineer. To-morrow the Engineer will merely describe the work the machinery he requires will have to do, and witness the tests of its performance. Some day he will become a mere inspector, the manufacturer supplying the scheme and the machinery required for its working from his own designs. I think this will be the inevitable result of giving men of science and high technical ability their proper places in the works; if so, the profession of Consulting Mechanical Engineer will not be as lucrative in the future as it has been in the past.

But if manufacturers wish to be freed from interference of Consulting Engineers with their designs and processes, they must give their customers full information about the machinery they tender for. Their specifications are often extremely vague. The specifications of engine-builders, for instance, are filled with such phrases as "great strength," "ample bearing surface," "best materials,"

"highest class of workmanship"—none of which convey the slightest meaning to the customer or his Engineer. *They* want to know the stresses and pressures per square inch allowed, the materials to be used for the various parts, which surfaces are to be machined, and what tolerances are allowed. For lack of information of this kind I have been compelled to make acceptance of tenders conditional upon the supply of a complete set of blue prints for examination, and upon consent to alter them at my request. All this involves expense, irritation, and serious risk of error. I think it would be better if all engine-builders built their own standard engines, as some do now, and refused to alter them, but gave each customer a complete set of blue prints—as very few do now—marked (not necessarily with dimensions), but with stresses, loads, materials, and machining. If the engines were built on a proportional system, one set of prints, though necessarily not to scale, would serve for a whole series. The customer would then see what he was going to buy and the Engineer could "manufacture" instead of building each engine to a special design. Thereby much trouble and expense would be avoided.

The objection has been raised that if A sent out his drawings, B could copy them. Well, what if he did? A could copy B, and both would be gainers by exchanging their ideas. Else why does the Institution of Mechanical Engineers exist? I can remember when engines were built with strokes of 6 feet and $\frac{3}{4}$ inch, and screws were cut with fancy threads to keep the repairs in the builder's shop. What folly it all seems now. What folly this plea for secrecy in such matters as the drawings of a steam-engine will seem in a few years' time. The secret of success in mechanical engineering lies, in these days of technical education, in quality and treatment of materials, accuracy of machine-tools, and skill of workmen, not in drawings.

I have been led into this digression by reflecting on my own experience. I return to the more general question of organizing the Engineering Trade both in its individual workshops and as a whole.

Except in a few cases, workshop organization here has not

received the attention given to it in America and Germany. There are still shops without definite planning of the progress of the work, without adequate equipment of jigs and gauges, and without standard shapes of tools or a tool-room; where men drift about in search of tools and tackle or wait in idleness for drawings or materials, where machinery is obsolete, and light so bad that good work could not be done if the machinery were up to date.

Such shops will have to go. They cannot compete in price or quality of work with those where, what is known as "scientific management," or anything approaching it, prevails; where the progress of every job is planned to the last detail before it is sent into the works; where machinery is so arranged that each piece passes through the whole series of operations to be performed upon it in predetermined order and without pause, and is immediately succeeded by another piece to undergo the same cycle of operations; where labourers and tackle for fixing the work in the machines are ready the moment they are wanted; where drawings, gauges, tools properly ground to standard shapes come with the work; where cleanliness, light, and comfort reign, and where endeavour is made to get the workman to regard his work more as a problem to be solved than as a task to be got through.

And if the industry is insufficiently organized in machinery and shop management, it is still more so in the equally important business of obtaining orders to keep the shops at work. There are no central selling agencies like that of the great thread "Combine." Each firm has its own agents. Many of those working in England have little or no engineering knowledge, and those working abroad are often without the social influence and powerful diplomatic and financial backing of their competitors. We are far too much inclined to look upon the commercial representatives of engineering firms as "bagmen." I have been humiliated and indignant at the supercilious discourtesy or downright insolence with which I have sometimes seen these men received, even by Engineers. While they are treated in this way the right class of men will never enter this branch of the engineering industry. To represent the industry abroad we need cultivated men as well as

technical experts, tactful diplomatists, masters of other languages than their own, and the social equals or superiors of those they may have to transact business with. Small firms cannot afford to employ such men any more than they can afford to keep their machinery up to date, or to refuse orders for goods which their machinery is not specially adapted to produce, to provide instruction for their apprentices, to pay for scientific assistance, or to undertake experiment and research. For these and other reasons there is not time to explain now, some kind of combination or co-operation among engineering firms, especially the smaller firms, seems very desirable, if not indeed essential.

Combinations have been arranged in other trades. The Fine Spinners and Doublers Association, Associated Portland Cement (Limited), Wall-paper Manufacturer (Limited), United Alkali (Limited), the North British Locomotive Company, are examples. None of these except the last is engaged in engineering, but they may be used as precedents to overcome the objection to development, the *omne ignotum pro horribile* sentiment which is so ingrained in the English character. In Germany, I understand, the dye trade, and all the trades ancillary thereto, have just been combined into one vast concern, with a capital of 800,000,000 marks. The great and powerful combinations in the steel trade in America and Germany cannot be ignored. The industrial world does not stand still. Similar combinations among engineers will have to be seriously considered. The Socialistic ideal of Government control and ownership, though it may in some degree be necessary to carry an unprepared, unorganized nation through a great war, will never commend itself to the people of this country. Mechanical Engineering to be efficient must be independent, and to be independent it must have some powerful representative body to decide and voice its policy. Much thought has been, and is now being, given to the formation and functions of representative Trade Associations, and several such Associations have been formed directly or indirectly connected with Mechanical Engineering. It is sufficient for my purpose to mention two of them.

First, the Federation of British Industries, the child of Mr. Dudley Docker, C.B. It is intended to represent all the industries of the country.

Second, the British Engineering Association (the B.E.A.) connected with the name of the inventor of that very efficient "strafing" apparatus, the Stokes Gun. With it is now amalgamated the Council for the Organization of British Engineering Industries, born at the Manchester Engineers' Club in 1914. The Amalgamation is intended to be representative of the entire engineering industry, just as another similar Association, the British Electrical and Allied Manufacturers' Association (the BEAMA), is intended to represent the electrical industry.

In these Associations might be found the components of a practical and effective scheme for the organization of the British engineering industry, if only it were possible to co-ordinate them, to induce all firms to belong to them, and to get the Trade Unions to co-operate.

For instance, at one end of the chain there might be the individual employer responsible to the District Executive of the British Engineering Association for the management of his own works, the corresponding Trade Union authority being the Shop Stewards or the men's Shop Committee.

The next link would be the District Executive of the British Engineering Association, with which might be amalgamated the District Committee of the existing Engineering Trades Employers' Federation. This District Executive would be charged with the oversight of the district employers, works, and trade. In co-operation with the district officers of the Trade Unions they would arrange rate of wages and piece-work, terms of co-partnery, the number of apprentices for whom places could be found and the means of continuing their education. The District Executive would receive reports from employers as to the state of their order books, their difficulties with labour, supply of materials, and transport; also as to experiments and research required to overcome local manufacturing difficulties, and would report to the Executive Council of the B.E.A., i.e. the Executive Council of the

entire engineering industry. If combination or amalgamation could be arranged they would also advise the Executive Council of the B.E.A. as to the kind of work each shop in each district could profitably undertake, what machinery required replacement, what works should be removed to other districts or closed altogether; they would also keep reactionary employers in order.

The next link—the supreme body in the Engineering trade—might be the Executive Council of the British Engineering Association. With it might be amalgamated the existing Engineering Trades Employers' Federation. Its proposed activities can be partly gathered from what I have just said. They are described at length in a report presented to the Council for the Organization of British Engineering Industry at the Engineers' Club in Manchester in November 1915. I cannot repeat them here. They include transport, intelligence, production, patents, publicity, finance, education, research, and legislation in the broadest sense. The Council would act through standing Committees. Its authority would be great and its influence far-reaching. The corresponding body acting for the men would be the Executives of all the Engineering Trade Unions.

Last of all would come the Federation of (all) British Industries and the Federation of Trade Unions, if that body be brought to birth. These would deal directly with a Minister of Industry and Commerce. The chain would then stand thus:—

The Manufacturer.	=	The Shop Stewards.
The district Executive of the B.E.A.	=	The district Officers of the Engineering Trades Unions.
The supreme Executive Council of the B.E.A.	=	The Executives of the Engineering Trade Unions.
The Federation of (all) British Industries.	=	The Federation of Trade Unions.

The Minister of Industry and Commerce.

As yet there is no Minister of Industry and Commerce, but there is a Minister of Labour. Labour is a very essential element

in industry. Both have the same interests. Let us not have two Government Departments to squabble with each other and accentuate the difficulties which we hope to smooth away. Let the Ministry of Labour absorb all interests and become the Ministry of Industry and Commerce, acting for the interests of employers and employed alike.

I have outlined this scheme of co-ordinated Associations as a speculation. I do not suppose there is the faintest chance of its adoption. I fear that many manufacturers would oppose it tooth and nail. I have no idea how the Trade Unions would regard it. But even if the combination or amalgamation suggestions be deleted, there would still remain a skeleton on which a living body of some kind might be built. And here to prevent any misunderstanding, I think I ought to say that the Manchester report, to which I have referred, does not advocate a "combine"; on the contrary, it disclaims any such intention. I have suggested it myself because I cannot see that much good can be done unless at least the small firms be amalgamated. Moreover, the suggested amalgamation of the existing Engineering Employers' Federation with the new B.E.A. might not be easily accomplished.

Employers and Employed.

I have spoken so far of matters which we Engineers can deal with if we will, our own education and training, the education and training of our workmen and the organization of our industry. But now I come to a problem of far greater difficulty, the negotiation of a treaty to secure a lasting peace between employers and employed. Without this peace the victory over Germany, for which we have sacrificed so much, will be in vain. The future of the British Empire depends on its attainment.

Even if it were not so, a treaty should be made. The Government and all firms working under it have bound themselves jointly and severally to restore all the Trade Union restrictions, rules and customs which the Unions, at the request of the Government, agreed to abrogate for the duration of the war. Their restoration has been a condition in all Government contracts since March 1915,

and was guaranteed by the Prime Minister in the House of Commons in the following month.*

I hope the Trade Unions will have learnt, before the war is over, that a policy which compels a workman to limit his output and his earnings, which refuses a manufacturer a due return on the cost of installing up-to-date machinery, and on expenditure for scientific research, is as injurious to the workman as to his country, and that if persisted in it will ultimately ruin both, even if it bring particular Unions some temporary gain. If we are to compete successfully with nations in whose industrial vocabulary "ca' canny" finds no place, limitation of output and restrictions on working machinery will have to be given up.

But if the Unions be wise enough to give up voluntarily what we have undertaken to restore, we are in honour bound to give them some acceptable equivalent. I have seen in the newspapers that conferences are being held between representatives of the Employers and the Unions with a view to the negotiation of a treaty of industrial peace. I hope that the report is true, for I believe that more industrial disputes are caused by misunderstandings than by wrongs, and that misunderstandings most easily become understandings when men meet face to face resolved to find the "middle way."

Of the possible or desirable terms of such a treaty, it is not my place to speak. But I may try to influence the spirit in which the questions at issue should be discussed and in which the inevitable difficulties of interpretation and execution of the terms agreed on should be faced. I want it to be a spirit of sympathy and great forbearance. I know that I am asking much. The strikes, the deliberate limitation of output by some men, the

* On the 23rd June 1915, Mr. Asquith said: "We promised we would give a guarantee that at the end of the war the pre-war conditions would be restored. We have done it not merely by solemn declarations on the part of the Government, but we have embodied them in an Act of Parliament. We have a statutory guarantee carried unanimously by Parliament by men of all parties—all sorts and conditions of men. They are all in it and they are all committed to that guarantee."

bargains driven with the Government in the hour of the country's direst peril, have roused an indignation difficult to control. I ask for sympathy and forbearance nevertheless. I think that many of the men, and of their leaders, have done their best in very difficult circumstances, and where they have been to blame, I think, before passing judgment, we should consider how far we are ourselves responsible for the conduct we so bitterly condemn.

For 100 years employers and employed have been at war, and war leaves bitter memories. The war began when the tools of his trade were transferred from the workman to his master and he became dependent on his master's will for permission to use them. In 1817 trade was bad and employment difficult to obtain. The country was exhausted by the Napoleonic Wars. Food was scarce and dear. Wages were low and largely paid in goods sold in the masters' shops and at the masters' prices. Children of tender age were driven to labour in the mills.* Houses and factories were insanitary, and many trades were deadly to the workers. There was no legislation to guard life or limb. When men were killed in collieries no inquests were held. Ignorance was dense. Thousands could neither read nor write. The only schools for the people were the Church of England parish schools. Travelling was difficult and costly. Means of finding employment or learning rates of wages and conditions of labour outside a man's immediate neighbourhood were beyond the workman's reach.

Nor was his material condition the sole cause of the workman's discontent. The law forbade him to improve his lot. In 1817 "any persons combining with others to advance their wages or decrease the quantity of their work, or in any way affect or control those who carried on any manufacture or trade in the control or management thereof, might be convicted by one Justice of the Peace, and might be committed to the common gaol for any time

* Extract from a protest against child labour in 1816. "Seven years is the earliest age at which children ought to be employed in factories, and then only for 4 or 5 hours per day. At eight or nine years of age they might be employed for 6 or 7 hours, afterwards they might be employed 10 hours, and beyond there should be no increase of labour."

not exceeding three calendar months, or be kept at hard labour in a House of Correction for a term of two calendar months."

Is it astonishing that men, bred up in ignorance and deprived of free speech by law administered by their oppressors, were driven by anger and despair to violence? Indeed, the injustice of the law was so apparent that a Select Committee of the House of Commons reported in 1824, that the existing law had not only not been efficient to prevent combinations either of masters or workmen, but on the contrary had, in the opinion of both parties, had a tendency to produce mutual irritation and distrust, and to give a violent character to the combinations, and to render them highly dangerous to the peace of the community. The Committee also expressed the opinion that masters and workmen should be freed from such restrictions as regarded rates of wages and hours of working, and be left at perfect liberty to make such agreements as they mutually thought proper; and they recommended that the statute law, which interfered in this particular between masters and workmen, should be repealed, and also that the common law, under which a peaceable meeting of masters or workmen might be a ground for prosecution, should be altered.

An Act giving effect to these recommendations was passed in 1824, but was repealed in 1825, because, as the preamble of the repealing Act set out, such combinations as were legalized by the Act of the previous year "were injurious to trade and commerce, dangerous to the tranquillity of the country, and especially prejudicial to the interests of all concerned in them."

Such was the oppression and injustice under which the workers toiled and the masters made their fortunes when our grandfathers were young; yes, and also when the present workers' grandfathers were young, and the grandsons have heard, and have not forgotten the tales of their grandfathers' young days. The first Truck Act forbidding payment of wages in food, in drink, in clothes, or articles other than current coin, and making illegal all agreements as to where the money should be spent, was only passed in 1831. Nor has it been forgotten that, with the exception of a few men like Robert Owen and Michael Sadler, employers as a class for many

years consistently opposed all legislation making for the health and safety of the workers. The workers know, too, that some employers refuse to recognize the Unions even now.

That the Unions have roused hostility by the intolerance of some of their officials and by their selfish economic policy is unfortunately true. But in our attitude towards them we should not forget that intolerance is the natural reaction of inexperienced youth in possession of unlooked-for power against past oppression and injustice, or that an unwise policy is the fruit of ignorance. The A.S.E, one of the oldest of the Trade Unions, was only founded in 1851. The first Elementary Education Act was passed in 1870.

Employers also should consider how far limitation of output, and objections to piece-work profit-sharing and co-partnership, two of the most serious causes of difference with the Unions, are due to their own short-sighted policy in cutting piece-work rates. The case has been very plainly put by Mr. George Barnes, M.P. "The system," he says, "has been a failure because, in the stress of competition, piece-work earnings have tended to slide downwards to what previously had been a time-work wage." Indeed, it would almost seem to have been an article of economic religion that a workman's earnings should be limited by precedent. And thus we go on in a vicious circle, the master unable to increase wages because the workman will not give the necessary output, the workman limiting the output because the master does not raise the pay.

But that is not the only reason why the workman limits output and refuses piece-work. Deeply in his mind is rooted the belief that increased output causes glutted markets, short time, and unemployment, a terror which casts its shadow over his whole life. This delusion can only be removed by education.

I have said all this, not to raise prejudice against employers, but to show the ground on which I am appealing for understanding and forbearance in the great effort which I hope is to be made to root out the suspicion and ill-will which, before the war, was settling as a deadly blight upon the industries of England, and is

even now weakening our fighting power. We have to convince the workers by experience, and teach them by education, that piece-work, co-partnery, and large output are to their advantage, and that only by putting the best machinery to the best use can high wages, relatively to the cost of living, be maintained.

To-day they are getting an object lesson which they must not unlearn through any act of their employers. The Prime Minister alluded recently to the subject in the House of Commons, and his words should be marked, learnt, and inwardly digested by employers and employed alike :—

"I am convinced," he said, in connexion with shipbuilding, "after a great deal of examination of the problem, and hearing what has been said by both employers and working-men, that the output can be considerably increased by an alteration of methods. Wherever payment by results has been introduced there has been an increase in the output of shipbuilding yards, sometimes by 20, sometimes 30, and sometimes even 40 per cent. We have made a special appeal to the great Trade Unions concerning shipbuilding to assent to the introduction of these methods. In some yards they have already been introduced. I was very glad to get a telegram from Liverpool stating that the Liverpool Trade Unions had agreed to recommend to their workmen that that course would be pursued. It also involves an undertaking on the part of the employer that if large wages are made by men as a result of special effort no advantage must be taken of that to reduce the rates, and I am perfectly certain from what they told me that they have no intention of doing anything of the kind. And if they do I want them to feel that not merely the Government would deprecate, but the House of Commons would be behind the Government in deprecating, any attempt to penalize the good worker by reducing the rate of payment. *That has been the great folly of the past, and that is why the fear of these piece-work rates has simply bitten into the workmen's mind.*"

The italics are my own. And here I venture to interject a timely caution. Directly the War is over, the Engineering trade will have a period of activity without precedent; wages will, if possible, rise, and enormous profits will be made. These profits must not be distributed in inflated dividends, as in the "boom" after the Franco-Prussian War, first because large dividends will encourage speculative enterprises whose failure will hasten and

accentuate the financial stringency and bad trade which must come when the first great boom has to be liquidated; and secondly, because the money must be saved to keep up wages and employment when the bad time comes and the employers' theory of piece-work is put to the test.

To-day piece-work prices are fixed in some places by associations of employers and employed, but generally, I think, the former have had and still have the business practically in their own hands. In the future I hope that not only piece-work rates, but wages, demarcation, and other contentious questions will be settled jointly by one or other of the Associations I have mentioned. Surely with such an organization managed in its higher courts by men of ripe experience and ability it would be possible to arrive at just decision and to control the headstrong and unreasonable among the employers and the men.

Then there is another matter which will have to be considered soon—the length of the working day. We ask for better education of the workers. We mean to try to give it them. But to enjoy its fruits they will demand more leisure, and I think public opinion will uphold them. The eight-hours day has been proposed and tried, and if the Unions will allow the men to do their best it may be possible, even in the face of the coming competition, to keep wages up, in spite of the shorter working hours. It must be remembered, however, that in the Engineering trade standing charges and interest on cost of machinery amount to several times the wages bill, and that many of these charges run on while works are standing idle, adding to the cost of every article produced and reducing the margin available for increasing wages.

Would it be possible to introduce a two-shift day? A morning shift, and an afternoon and evening shift, by which the men would get one fortnight, free mornings; the next, free afternoons; every day their cherished midday meal; and every night in bed. A three-shift day is worked at greater inconvenience, but having regard to the present attitude of the Unions, I fear the suggestion is outside the region of practicable politics for the present.

It is, perhaps, worth while mentioning that the three great disputes in the Engineering trade were concerned with one or other of the subjects I have mentioned, qualification of men to work machines, hours of work, and limitation of output. The objects and results of these disputes are worth consideration.

The lock-out of 1852 was brought about by a demand for the abolition of piece-work and the removal of labourers from machines. The masters were victorious.

The strike of 1871 was caused by the refusal of the employers to grant a fifty-four hours week. The men won.

The lock-out of 1897–1898, though it began with a strike on the question of an eight-hours' day, was really brought about by interference of the Unions with the management of the works, the selection and training of machine workers, objections to the employment of non-unionists; but above all, by limitation of output. The men were beaten.

In the first and last, public sentiment and right were with the masters, in the second with the men. I shall be told that the issues of these disputes were governed by the state of trade. And so they may have been in part, but I believe there is a power that works unceasingly through right and wrong to weave the tangled web of anarchy into the pattern of an ordered life, and that these ephemeral troubles are but means that it employs to lead both sides to a right conception of their duties to each other. That this enlightenment will come is sure. The ignorance of men, their prejudices and their greed, alone postpone its advent. The masters who sweat workers, cut piece-work prices, use obsolete machinery, crib time, and limit their care for health and comfort to the minimum requirements of the Factory Acts or less—the workmen who deliberately lose time and limit output, who refuse to allow machinery to be worked by men as competent to work it as themselves, who obstruct the introduction of new methods and new processes of manufacture—if we could ship them one and all to Germany to assert their "rights" against each other on a foreign soil and under Prussian discipline; or better far, if we could lead them side by side in mutual trust, to bury strife, suspicion, and

ill-will, deep in the graves of their sons and brothers who have fallen in the war, and teach them to see the right as clearly as their dead sons and brothers see it now—the ideal of industrial life would be in sight.

It is to this end that I plead for understanding and forbearance from employers. You cannot drive 2,000,000 Trade Unionists, but you can educate and persuade them. "In a national crisis men should think more of what they could give than of what they were likely to get," said a great Labour leader to the Manchester Trade Union Congress only three months ago. Can *we* not act upon his words —the time is ripe. We stand to-day upon a peak of circumstance uplifted by the War, our eyesight cleared by common peril, our sympathies unsealed by common pain. Behind us are the dark days of the past, before us on the far horizon is the new industrial world. Can we not turn our backs upon the night and march in faith, in hope, in charity, towards the dawn—the dawn of a new day when England shall be Merrie England in the noblest sense, wherein employer and employed shall live in peace and work together for the glory of their native land?

Sir ALEXANDER B. W. KENNEDY, F.R.S. (Past-President), in moving a hearty vote of thanks to the President for his Address, said the Members had already expressed in the most eloquent manner exactly what he desired to say, and it was very unnecessary for him to add anything after the great applause with which the President's Address had been received. As one of the seniors in the engineering world—at least, so far as Engineering Societies were concerned—he had had his share of moving votes of thanks to various Presidents, but he could honestly say that he had never performed that duty with so great a pleasure as on the present occasion. He would not say that his natural Highland pugnacity had not been somewhat aroused by some vigorous expressions of opinion about Consulting Engineers. But it subsequently appeared that the President, as himself a Consulting Engineer, did exactly what his other professional brethren did. And, in any case, any tendency towards pugnaciousness vanished altogether in his

admiration for the really noble Address to which they had listened. The President had expressed in a bold, striking, and straightforward manner views which, although they might not be universally accepted, were at any rate not unworthy of being universally accepted, and which represented a more reasoned, impartial, and sympathetic attempt to deal with the great industrial problems of the future than anything else he had read or heard. There were two or three specially important points in the Address to which he would have liked to refer, but he felt that it would only spoil the effect of the Address as a whole if he were to separate them out from the rest, and he would therefore simply content himself with asking the Members to show once more by acclamation their appreciation of the most notable Address which the President had given them.

Dr. William H. Maw (Past-President) said it afforded him the greatest pleasure to second the vote of thanks which had just been moved by his friend Sir Alexander Kennedy, who had admirably summarized the character of the Address to which the Members had had the pleasure of listening. Its distinguishing feature, which must have struck all the Members, was that it represented the plainly stated and straightforward convictions of an engineer of great experience, who had studied in all its aspects the development and present condition of the British engineering industry. As such, it merited most careful attention, and he considered that the Institution was extremely indebted to the President for having put his convictions before it in such an admirably clear and eloquent manner.

Sir Alexander Kennedy, in putting the Motion to the Meeting, said it might not be known to the Members that Mr. Longridge, in addition to being the President of the Institution, as well as an active professional Engineer, was doing responsible, difficult, and necessary, although sometimes thankless, work in connexion with the Munitions Inventions Department, and in this way was doing duties in connexion with the war which only an experienced engineer could undertake.

The Resolution was then put to the Meeting by Sir Alexander Kennedy, and carried with acclamation.

The PRESIDENT, in acknowledging the compliment, said the vote of thanks that had just been passed to him was especially welcome in that it had been proposed by Sir Alexander Kennedy, who was very nearly the oldest living President of the Institution, and because it had been seconded by Dr. Maw. He could not but confess that the last two months had been to him a time of some anxiety, first of all in deciding what the subject of his Address should be, and, after deciding it, wondering what sort of reception it would meet with. On the first point the Members had convinced him that he chose aright. What he had said had not aroused any hostility, but had been accepted in the kindliest manner.

In his treatment of the subject, he had been compelled to omit much for want of time. In connexion with education, he felt that he ought to have mentioned the question of research and the work of the National Physical Laboratory; and, in connexion with engineering organization, the work of the Engineering Standards Committee. That was a work with which Mechanical Engineers were especially bound up, and, like many other important subjects which were being dealt with at the present time, the Committee had had to undertake a large pecuniary liability, amounting to some £20,000, for the translation of their Specifications and their publication at a low cost, to compete with the quarter-dollar Specifications of the American Standards Bureau. A large part of the money had been furnished by the great generosity of one or two Members of the Institution, and by a large grant from the Government, but an amount of between £6,000 and £7,000 was still needed, and he thought the Institution should try and raise part of it. There were over 6,000 members of the Institution, and, if they would all do a little, the Institution's share of that deficit would certainly be wiped out.

AN ENERGY-DIAGRAM FOR GAS MIXTURE, AND SOME OF ITS USES.

By WILLIAM ALEXANDER, of the South African School of Mines and Technology, Johannesburg, *Associate Member.*

[*Selected for Publication, with Discussion in writing.*]

The great utility of energy diagrams in solving thermodynamic problems connected with proposed heat-engines, and in studying the thermal action of existing engines, is now fully recognized. For the working substances employed in the steam-engine and in refrigerating machines there are available excellent charts in the British and in the French system of units, which charts probably leave little to be desired in the matter of accuracy and convenience. Whereas for average gas mixture used in gas-engines no chart in British units has been published. A chart in French units has been published by Professor A. Stodola.* For those who prefer to work with British units of pressure and volume, the chart to be described has been prepared; and at the risk of some repetition of matter in other publications the construction, the theory, several of the

* *See* his book on "Steam and Gas Turbines," 4th edition, which has not yet been translated into English; also *see* Zeitschrift des Vereines deutscher Ingenieure, Vol. 10, 1913, pages 65–171. The Author has not seen, and has been unsuccessful in obtaining access to, either of these publications.

uses of the chart, and a few of the results obtained with it are explained.

In submitting such a chart it is important to adopt authoritative expressions for the variable specific heats of average gas mixture. Expressions due to Clerk,* the British Association Explosions Committee,† and Stodola ‡ are respectively

$$\left.\begin{aligned} k_v &= 0{\cdot}18 + 0{\cdot}000051\ \mathrm{T} \\ k_p &= 0{\cdot}251 + 0{\cdot}000051\ \mathrm{T} \end{aligned}\right\}, \qquad \left.\begin{aligned} k_v &= 0{\cdot}1515 + 0{\cdot}000075\ \mathrm{T} \\ k_p &= 0{\cdot}2225 + 0{\cdot}000075\ \mathrm{T} \end{aligned}\right\},$$

$$\left.\begin{aligned} k_v &= 0{\cdot}167 + 0{\cdot}000062\ \mathrm{T} - 0{\cdot}725 \times 10^{-8}\ \mathrm{T}^2 \\ k_p &= 0{\cdot}2377 + 0{\cdot}000062\ \mathrm{T} - 0{\cdot}725 \times 10^{-8}\ \mathrm{T}^2 \end{aligned}\right\}.$$

For all three cases $\mathrm{P}_v = 99\ \mathrm{T}$ approx., where T is the absolute temperature centigrade, r = volume in cubic feet per lb., P = pressure in lb. per square foot.

It has been shown by Captain T. B. Morley, D.Sc.,§ for the ideal Otto cycle with maximum temperature 1,600° C. abs. and compression ratio (volumes) of 6, that the respective efficiencies with these specific heats are 0·435, 0·44, and 0·45. These figures differ so little that for practical purposes any one of the three pairs of specific heats might be taken. The specific heats chosen for the chart dealt with in this Paper are Stodola's, as they are based on later research.

To enable the simplest expressions for the efficiencies of the ideal cycles of gas-engines to be employed, and to render the determinations from the chart as speedy as possible, all of the six state characteristics of the gaseous substance are represented.

Derivation of the Chart.—The functions that define the state of, say, a pound of substance are

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* "Limits of Thermal Efficiency in Internal Combustion Engines," by Dugald Clerk, Proceedings, Institution of Civil Engineers, vol. clxix.

† "The Internal Combustion Engine," by H. E. Wimperis, M.A., Constable & Co., Ltd.

‡ Zeitschrift des Vereines deutscher Ingenieure, 22nd June, 1912.

§ "The Limits of Efficiency of Gas Engines," *Engineering*, 18th December, 1914.

P, the pressure in lb. per square foot, or

p, ,, ,, ,, ,, ,, ,, inch,

v, the volume in cubic feet per lb.,

T, the absolute temperature (centigrade),

ϕ, the entropy,

E, the internal energy,

I, the total heat, defined by $I = E + \frac{Pv}{J} = E + APv$, where

$J = \frac{1}{A}$ = Joule's equivalent = 1,400 ft.-lb. per pound degree centigrade.

For a gas or gas mixture, any pair of these functions except those from the group EIT completely define the thermal state in a cycle of operations. In the chart,* Fig. 1 (page 336), the relation between these six functions is shown for a range of temperature from 300° C. abs. to 2,200° C. abs., although experiments on specific heats have not been carried to such high temperatures.

Relation between Total Heat and Temperature.—Let the general values for the specific heat at constant pressure and constant volume be

$$k_v = b + sT - uT^2; \quad k_p = a + sT - uT^2;$$

where a, b, s, and u are constants.

$$\text{The total heat} = I = E + APv = E + RT \quad . \quad . \quad (1),$$

where $R = k_p - k_v = a - b$. Therefore $dI = dE + (a - b)\, dT$.

Hence the change of total heat between any two states (one and two) is, by integrating k_v between the limits T_1 and T_2

$$I_2 - I_1 = b\,(T_2 - T_1) + \frac{s}{2}(T_2^2 - T_1^2) - \frac{u}{3}(T_2^3 - T_1^3) + (a - b)\,(T_2 - T_1)$$

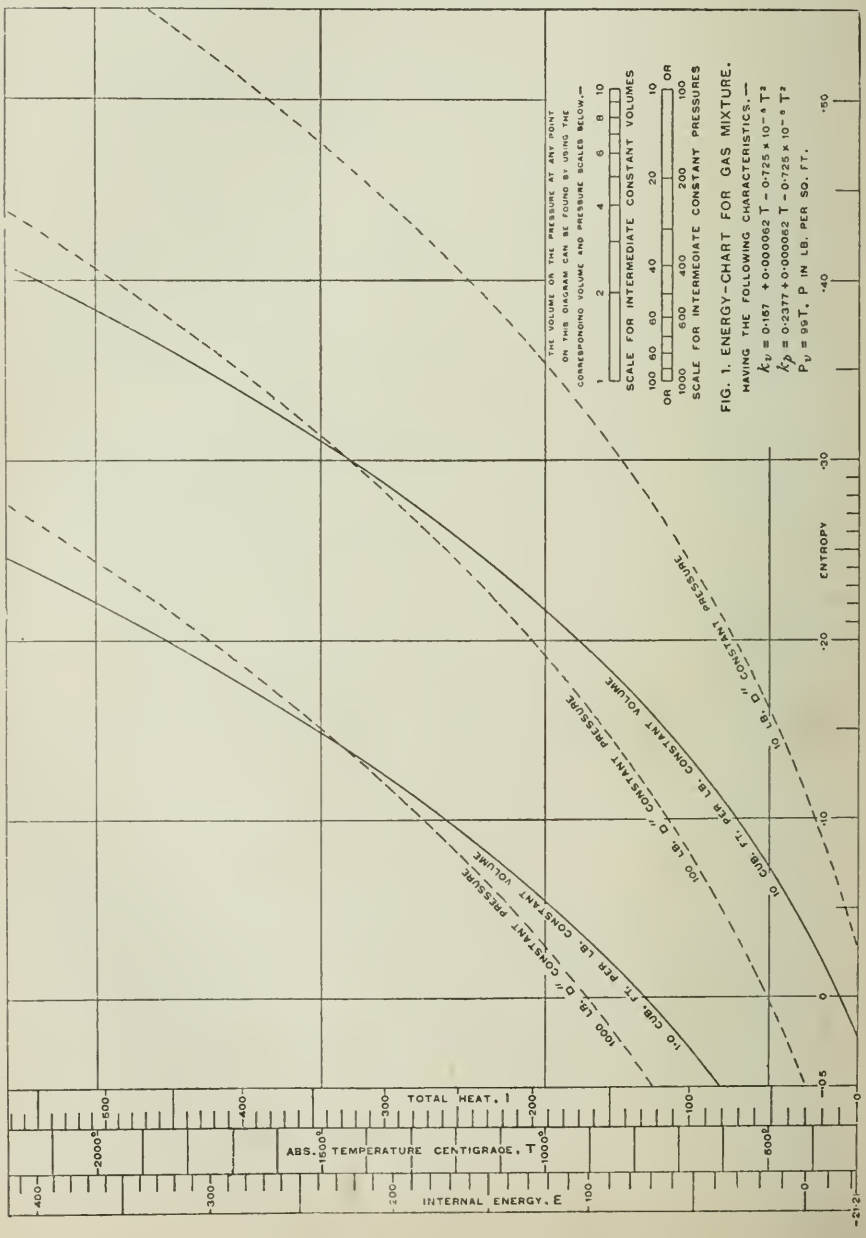
$$= a\,(T_2 - T_1) + \frac{s}{2}(T_2^2 - T_1^2) - \frac{u}{3}(T_2^3 - T_1^3) \quad . \quad (2),$$

which enables the scale of total heat to be calculated. The change of I is seen to be dependent only on the absolute temperatures, and is therefore independent of the process of changing volumes and pressures by which the gas passes from one state to another.

* The original Chart, 4·3 times larger and containing many intermediate curves, may be consulted in the Institution Library.

Fig. 1.—*Combined Chart of Entropy-Temperature* (ϕT) *and Entropy-Total Heat* (ϕI).

Note.—The original Chart may be consulted in the Institution Library.


THE VOLUME OR THE PRESSURE AT ANY POINT ON THIS DIAGRAM CAN BE FOUND BY USING THE CORRESPONDING VOLUME AND PRESSURE SCALES BELOW.—
SCALE FOR INTERMEDIATE CONSTANT VOLUMES
SCALE FOR INTERMEDIATE CONSTANT PRESSURES
FIG. 1. ENERGY-CHART FOR GAS MIXTURE.
HAVING THE FOLLOWING CHARACTERISTICS.—
k_v = 0·167 + 0·000062 T − 0·725 × 10⁻⁸ T²
k_p = 0·2377 + 0·000062 T − 0·725 × 10⁻⁸ T²
P_v = 96T, P IN LB. PER SQ. FT.
ENTROPY
TOTAL HEAT, I
ABS. TEMPERATURE CENTIGRADE, T
INTERNAL ENERGY, E
1 LB. CONSTANT PRESSURE
10 CUB. FT. PER LB. CONSTANT PRESSURE
100 LB. CONSTANT VOLUME
10 CUB. FT. PER LB. CONSTANT VOLUME
100 LB. CONSTANT PRESSURE

Relation between Internal Energy and Temperature.—From (1),

$$E = I - (a-b)T, \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$\therefore \quad E_2 - E_1 = I_2 - I_1 - (a-b)(T_2 - T_1) \quad . \quad . \quad (4).$$

Thus when the values of $I_2 - I_1$ are calculated from (2) the values of $E_2 - E_1$ are found from (4) for the internal energy scale.

Relation between the Entropy and Temperature for the Constant Volume and the Constant Pressure Lines.—If the heat added during a small operation be dQ, then

$$d\phi = \frac{dE}{T} + \frac{AP\,dv}{T} = \frac{dQ}{T} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

$$= k_v \frac{dT}{T} + R\frac{dv}{v} = (b + sT - uT^2)\frac{dT}{T} + (a-b)\frac{dv}{v}. \quad (6).$$

Again, since $APv = RT$, it follows that $AP\,dv + Av\,dP = R\,dT$. Thus, $AP\,dv = R\,dT - Av\,dP = R\,dT - RT\frac{dP}{P}$, giving by substitution in (5)

$$d\phi = \frac{dE}{T} + R\frac{dT}{T} - R\frac{dP}{P} = k_v\frac{dT}{T} + (a-b)\frac{dT}{T} - R\frac{dP}{P}$$

$$= (b + sT - uT^2)\frac{dT}{T} + (a-b)\frac{dT}{T} - R\frac{dP}{P}$$

$$= (a + sT - uT^2)\frac{dT}{T} - R\frac{dP}{P} \quad . \quad . \quad . \quad . \quad (7).$$

Since $d\phi$ is a perfect differential there results from (6) by integrating along constant temperature and constant volume paths,

$$\phi_2 - \phi_1 = b\log_e\frac{T_2}{T_1} + s(T_2 - T_1) - \frac{u}{2}(T_2^2 - T_1^2) + R\log\frac{v_2}{v_1} \quad . \quad (8)$$

and from (7) by integrating along constant temperature and constant pressure paths,

$$\phi_2 - \phi_1 = a\log_e\frac{T_2}{T_1} + s(T_2 - T_1) - \frac{u}{2}(T_2^2 - T_1^2) - R\log_e\frac{P_2}{P_1} \quad . \quad (9)$$

Therefore, for any constant volume line the difference between the entropies at any two temperatures T_2 and T_1 is

$$\phi_2 - \phi_1 = b\log_e\frac{T_2}{T_1} + s(T_2 - T_1) - \frac{u}{2}(T_2^2 - T_1^2) \quad (10), \quad .$$

and for constant pressure lines

$$\phi_2 - \phi_1 = a \log_e \frac{T_2}{T_1} + s(T_2 - T_1) - \frac{u}{2}(T_2^2 - T_1^2) \quad . \quad (11).$$

Also at any given temperature the difference of entropies for the constant volume lines v_1 and v_2 is

$$\phi_2 - \phi_1 = R \log_e \frac{v_2}{v_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad (12),$$

and for constant pressure lines P_1 and P_2 is

$$\phi_2 - \phi_1 = - R \log_e \frac{P_2}{P_1} \quad . \quad . \quad . \quad . \quad . \quad (13).$$

Taking zero of total heat and entropy at the lower temperature $T_1 = 300°$ C. abs., which is the approximate temperature at the end of the suction stroke in a gas-engine, equations (2) (4) (10) (11) (12) and (13) respectively reduce to

$$I = -74{\cdot}0248 + 0{\cdot}2377\ T + 0{\cdot}000031\ T^2 - 10^{-8} \times 0{\cdot}2416\ T^3 \quad (15),$$

$$E = I - 0{\cdot}0707\ T \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16),$$

ϕ, constant volume,

$$= - 0{\cdot}971 + 0{\cdot}167 \log_e T + 0{\cdot}000062\ T - 10^{-8} \times 0{\cdot}3625\ T^2 \quad (17),$$

ϕ, constant pressure,

$$= - 1{\cdot}3741 + 0{\cdot}2377 \log_e T + 0{\cdot}000062\ T - 10^{-8} \times 0{\cdot}3625\ T^2 \quad (18),$$

$$\phi - \phi_{(v = 14{\cdot}03)} = 0{\cdot}0707\ (\log_e v - \log_e 14{\cdot}03) \quad . \quad . \quad . \quad (19),$$

$$\phi - \phi_{(p = 14{\cdot}7)} = 0{\cdot}0707\ (\log_e p + \log_e 14{\cdot}7) \quad . \quad . \quad . \quad (20),$$

where p is now in lb. per square inch.

On the chart entropy is set off horizontally on an even scale, and an even scale of temperature and uneven scales of total heat and internal energy, the two latter being from (15) and (16), are set off vertically. By adopting an even temperature scale the advantages of the entropy-temperature diagram are realized, while the uneven total heat scale in no way detracts from the merits of the entropy-total heat diagram included in the chart.

Equations (17) and (18) show that the constant volume lines have all the same shape, inclination, and position in the vertical

direction, and so have the constant pressure lines, so that any volume or pressure line can be obtained from another by shifting horizontally by an amount that can be determined from (19) and (20). The constant volume line $v = 14{\cdot}03$ cubic feet, and the constant pressure line $p = 14{\cdot}7$ lb. per square inch, are first calculated from (17) and (18) and drawn. Then from (19) and (20) are found the differences in entropy (at the same temperature) between volume lines 1 and 10. This horizontal distance is divided logarithmically to give other volume lines. The same logarithmic division is employed to give the positions in the horizontal direction of the constant pressure lines.

It should be noticed from (15) and (16) that

$$\text{I abs.} = \text{I from chart} + 74{\cdot}025, \text{ and}$$

$$\text{E abs.} = \text{E from chart} + \text{I at } 300^\circ \text{ C. abs.} - 0{\cdot}0707 \times 300$$

$$= \text{E} + 74{\cdot}025 - 21{\cdot}2 = \text{E} + 52{\cdot}815.$$

Also, the absolute entropies corresponding to heating at constant volume and constant pressure are from (17) and (18) respectively

$$\phi \text{ abs. } (v = \text{const.}) = \phi \text{ from chart} + 0{\cdot}971,$$

$$\phi \text{ abs. } (p = \text{const.}) = \phi \text{ from chart} + 1{\cdot}3741.$$

Having obtained the chart, expressions may be given for the work done and the transfer of heat between the gas mixture and its envelope during different operations.

The advantage in representing the six functions of state will be evident from the simplicity of the expressions. The transfer of heat during a change from one state to another state, Figs. 2 and 3 (page 340), at constant pressure (AD) is $\mathrm{I}_d - \mathrm{I}_a$,* at constant volume (BD) is $\mathrm{E}_d - \mathrm{E}_b$, at constant temperature (AC) is $(\phi_c - \phi_a)\,\mathrm{T}_{ac}$.

The work done at constant pressure is $\mathrm{I}_d - \mathrm{I}_a - \mathrm{E}_d + \mathrm{E}_a$, at constant temperature is $(\phi_c - \phi_a)\mathrm{T}_{ac}$, adiabatically (CD) is $\mathrm{E}_d - \mathrm{E}_c$, in any expansion from D to F is $\mathrm{E}_d - \mathrm{E}_f + \mathrm{A}^1$, where A^1 is the area on the entropy-temperature diagram between the operation line DF and the axis of entropy at absolute zero temperature.

* *See* Author's Communication (page 352) for proof.

In Fig. 2 the area GDCH is represented by $I_d - I_c$, ACHG by $I_a - I_c + (\phi_c - \phi_a)\, T_{ac}$, GDFH by $I_d - I_f + A^1$, ADB by $I_d - E_d - I_a + E_a - (\phi_b - \phi_a)T_{ab}$, KDC by $E_d - I_c + I_k - E_k$.

The index n, of adiabatic expansion from D to C, in the very nearly exact equation $pv^n = c$, has the value AC ÷ BC, Fig. 3, or the value $(I_d - I_c) \div (E_d - E_c)$.

The exact equation can easily be shown to be

$$pv^{\frac{a}{b}}\, e^{\frac{(2sT - uT^2)}{2}} = c,$$

c being the base of the Napierian logarithms.

FIG. 2.

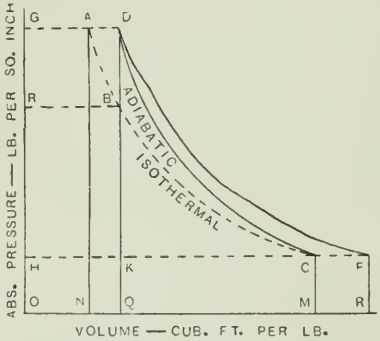
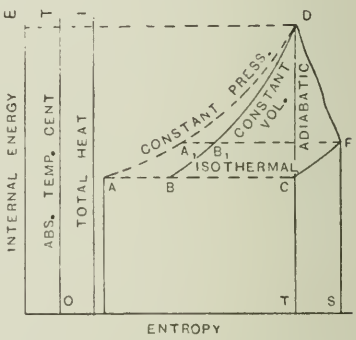


FIG. 3.



For any fairly regular expansion represented by DF, say, the average index n_1 is given by $A_1F \div B_1F$.

The efficiencies of the different cycles shown in Figs. 4 and 5, which have corresponding letters, are

$$\text{Otto cycle, eff.} = \frac{\text{DABC}}{\text{DAWD}} = \frac{E_a - E_d - E_b + E_c}{E_a - E_d}.$$

$$\text{Diesel cycle, eff.} = \frac{\text{EABC}}{\text{EAWE}} = \frac{I_a - I_e - E_b + E_c}{I_a - I_e}.$$

$$\text{Atkinson cycle, eff.} = \frac{\text{DAGHC}}{\text{DAWD}} = \frac{E_a - E_d - E_g + E_h - I_h + I_c}{E_a - E_d}.$$

$$\text{Cycle DAKCD, eff.} = \frac{\text{DAKCD}}{\text{DAWD}} = \frac{E_a - E_d - I_k + I_c}{E_a - E_d}.$$

$$\text{Cycle EAKCE, eff.} = \frac{\text{EAKCE}}{\text{EAWE}} = \frac{I_a - I_e - I_k + I_c}{I_a - I_e}.$$

The Practical Thermal Diagram.—Besides simplifying calculation in finding the efficiency of ideal diagrams, the energy-chart affords a convenient means of ascertaining to some extent the kind of thermal action that goes on in the cylinder of actual gas-engines, and enables a fairly close idea of actual efficiencies to be arrived at. To substantiate this a few practical cases are considered. An example is taken from a test of a Crossley engine, size 11½ inches by 21 inches, by Mr. Atkinson.* Neither the weight of gas in the cylinder nor the temperature at any one

Fig. 4.

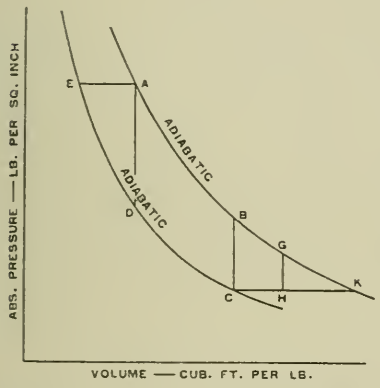
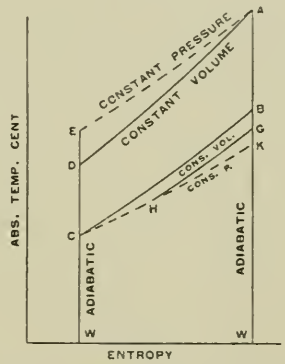


Fig. 5.



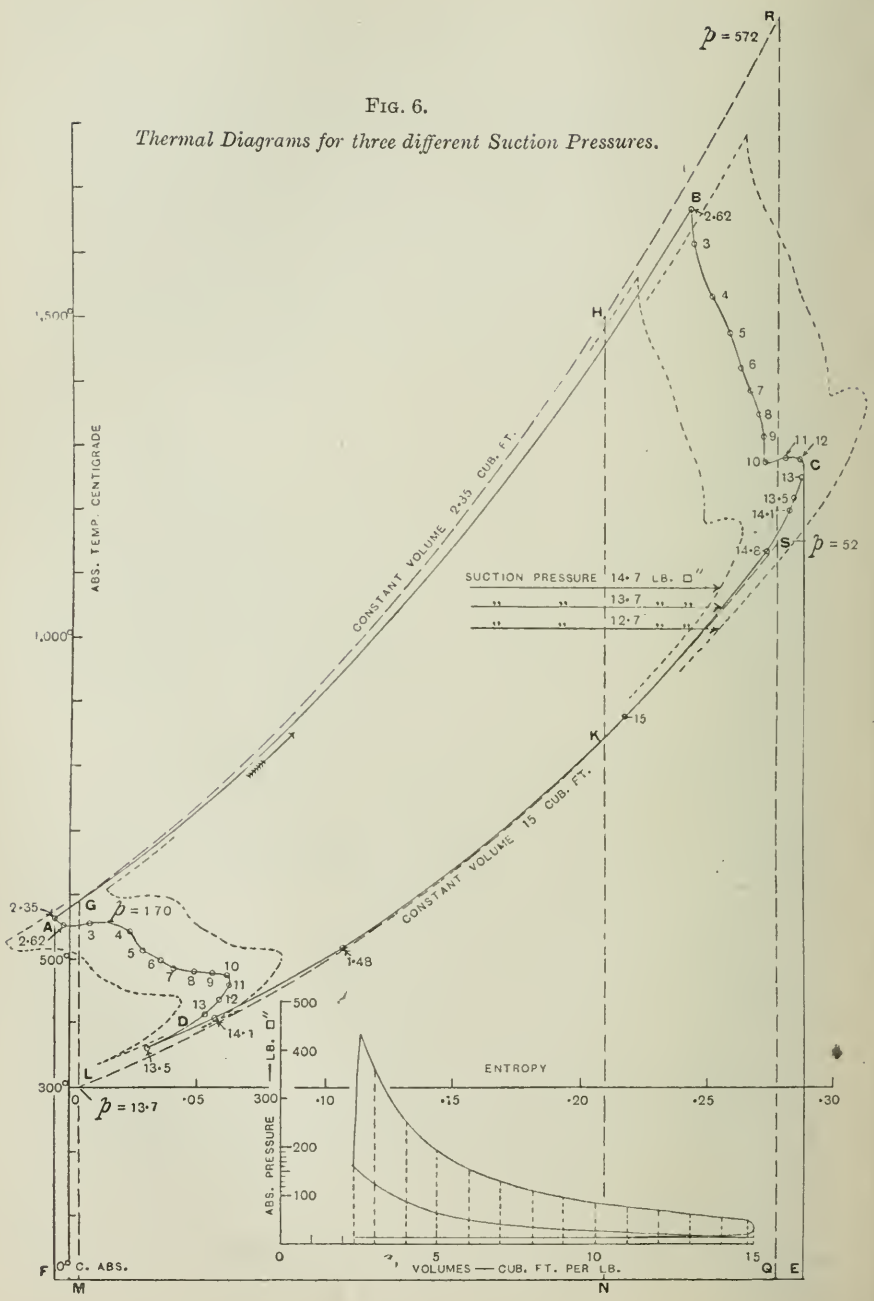
pressure is given in the Report; thus, to enable the indicator diagram to be plotted on the energy chart, the suction conditions have to be assumed, but any small error in this will be shown to have no appreciable effect on efficiencies.

The compression ratio was 6·37 (volumes). Taking the suction temperature 300° C. abs. in each of three cases where the suction pressures are assumed 12·7, 13·7, and 14·7 pounds per square inch, the corresponding volumes taken from the chart are 16·1, 15, and 14·03 cubic feet per pound of gas mixture. The volume

* "The Gas, Petrol and Oil Engine," by Dugald Clerk, D.Sc., and G. A. Burls, pp. 33 and 43; Longmans and Co.

FIG. 6.

Thermal Diagrams for three different Suction Pressures.



scale of the indicator diagram is obtained by dividing the total length from the toe of the diagram to zero volume axis into the same number of equal parts as there are cubic feet per pound at the end of suction under the assumed conditions. Fig. 6 shows this done for the suction pressure 13·7 pounds per square inch; the thermal diagrams are also given for the three suction pressures.

The ratio of area ABCD to area ABCEFA may be called the *practical cycle efficiency*, or simply the *cycle efficiency*. From the three diagrams, from the smallest upwards, the values of the cycle efficiency are 0·360, 0·359, 0·357, and the efficiency is therefore not much affected by the usual variations of suction pressure that occur between engine and engine. In the test the *indicated thermal efficiency* was 0·3535, while Professor Hopkinson * obtained the figure 0·368 in a test on the same type and size of engine. In both tests coal gas was the fuel. These figures show quite close agreement between the cycle efficiency and the indicated thermal efficiency.

The Mean Pressure from the practical Thermal Diagram.—It is interesting to compare this mean pressure with that obtained from the indicator diagram, as thereby a check is made on the accuracy of plotting.

The area ABCD on the original thermal diagram,† Fig. 6, is 300 cm.^{2} Now one square cm. on the thermal chart is equivalent to 543 ft.-lb. Thus the work done per pound per cycle = 543 × 300 = 163,500 ft.-lb. The change of volume during the cycle = total volume per pound at outer end of stroke less the volume of the compression space per pound = 15 − 2·35 = 12·65 cubic feet.

Hence the mean pressure $p_m = 0 \cdot 163500 \div (12 \cdot 65 \times 144) =$ 89 lb. per square inch. The same pressure is, of course, given by the smallest and largest diagrams in Fig. 6, because, although the

* "The Gas, Petrol and Oil Engine," by Dugald Clerk, D.Sc., and G. A. Burls, pp. 33 and 43; Longmans and Co.

† That is, as originally drawn—not on the reduced figure in the Paper. The original diagram was drawn to the scales of 100° C. per inch, and 40 inches per entropy unit.

area of the diagrams is different, the volume per pound is different in the same proportion, depending on the suction pressure. The mean pressure from the indicator diagram is 89 lb. per square inch.

To enable an ideal diagram to be obtained there is set off below the constant volume line 2·35 cubic feet (volume of compression space per pound) the area MGRQM equal to the area FABCE. This area, supposing the specific heats to be correct, is equivalent to the heat actually received by the gas mixture per cycle, and is equal to the chemical heat less the heat of dissociation in the exhaust gases and the heat given to the jacket and piston. The corresponding ideal diagram of thermal work is LGRSL with the lower boundary the constant volume line 15 cubic feet.

$$\text{The ideal efficiency} = \frac{\text{LGRSL}}{\text{MGRQM}} \quad . \quad . \quad . \quad (a)$$

$$= \frac{E_r - E_g - E_a + E_l}{E_r - E_v} = \frac{358 - 33{\cdot}8 - 156 - 21{\cdot}21}{358 - 33{\cdot}8}$$

$$= 0{\cdot}451.$$

$$\text{The cycle efficiency ratio corresponding to } (a) = \frac{0{\cdot}359}{0{\cdot}451} = 0{\cdot}798.$$

If the ideal cycle be taken as that in which there is the same maximum pressure as in the actual case, the ideal efficiency of this cycle GHKLG, Fig. 6, is

$$\frac{\text{GHKLG}}{\text{MGHNM}} = \frac{239 - 33{\cdot}8 - 88{\cdot}5 - 21{\cdot}21}{239 - 33{\cdot}8} = 0{\cdot}463 \quad . \quad . \quad (b).$$

$$\text{The cycle efficiency ratio corresponding to } (b) = \frac{0{\cdot}359}{0{\cdot}463} = 0{\cdot}778.$$

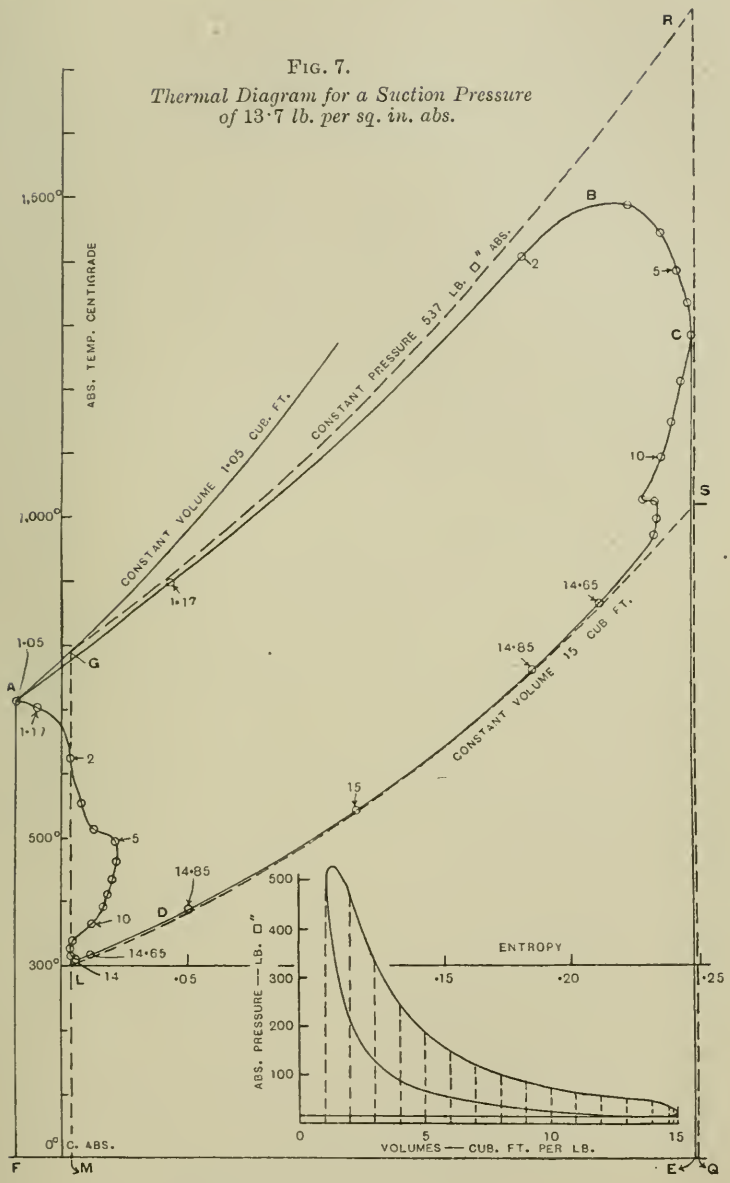
The air standard efficiency is 0·522, and the cycle relative efficiency is $0{\cdot}359 \div 0{\cdot}522 = 0{\cdot}687$.

Incidentally it may be noticed that the values for the ideal efficiencies (*a*) and (*b*) show clearly the well-known effect, when the specific heat is variable, of increase of temperature in lowering efficiency with a given compression ratio; whereas with the air cycle and air of constant specific heat, the efficiency is independent of temperature.

Another example is taken from a test by Professor Dalby of a

FIG. 7.

Thermal Diagram for a Suction Pressure of 13·7 lb. per sq. in. abs.



small Diesel engine,* $6\frac{1}{2}$ inches by 10·625 inches. The ratio of compression was 14·3. When the diagram was taken the speed was 259 revolutions per minute, and there was 10·66 i.h.p. being developed, this being 0·92 of full power. The thermal diagram, Fig. 7, has been found, as in the last example, for a suction pressure of 13·7 lb. per square inch absolute.

The cycle efficiency $= \frac{\text{ABCDA}}{\text{ABCEFA}} = \frac{350 \text{ cm}^2}{783 \text{ cm}^2} = 0{\cdot}447$. The indicated thermal efficiency from the test $= 0{\cdot}444$. These figures agree as well as those for the Crossley engine. The ideal diagram is found in the same way as before by setting off the area MGRQM equal to the area ABCEFA $= 783$ cm^2 on the chart, the top line being one of constant pressure 537 lb. per square inch absolute, which is reached after adiabatic compression from volume 15 cubic feet to volume $15 \div 14{\cdot}3 = 1{\cdot}05$ cubic feet.

$$\text{The ideal efficiency} = \frac{I_r - I_y - E_s + E_l}{I_r - I_y}$$

$$= \frac{436 - 132 - 125 - 21{\cdot}21}{436 - 132} = 0{\cdot}518.$$

Therefore, the cycle efficiency ratio $= 0{\cdot}447 \div 0{\cdot}518 = 0{\cdot}863$. The air standard efficiency $= 0{\cdot}62$; thus the cycle relative efficiency $= 0{\cdot}447 \div 0{\cdot}62 = 0{\cdot}72$.

From the thermal diagram ABCDA the mean pressure is 94 lb. per square inch, and from the indicator diagram 94·8 lb. per square inch, which is higher than Dalby's figure 92, due to slight errors in enlarging the small indicator diagram published.

The near agreement in these two examples, and in other cases that have been investigated, between the thermal efficiencies obtained from the test and from the energy chart shows that the latter method of obtaining efficiency is probably reliable enough for most purposes. If this be so, the method is valuable because it is inexpensive and saves much time.

In Fig. 8 the dotted diagrams are based on an indicator

* "Trials of a Small Diesel Engine," Proceedings, Institution of Naval Architects, 2nd April, 1914, or *Engineering*, 10th April, 1914.

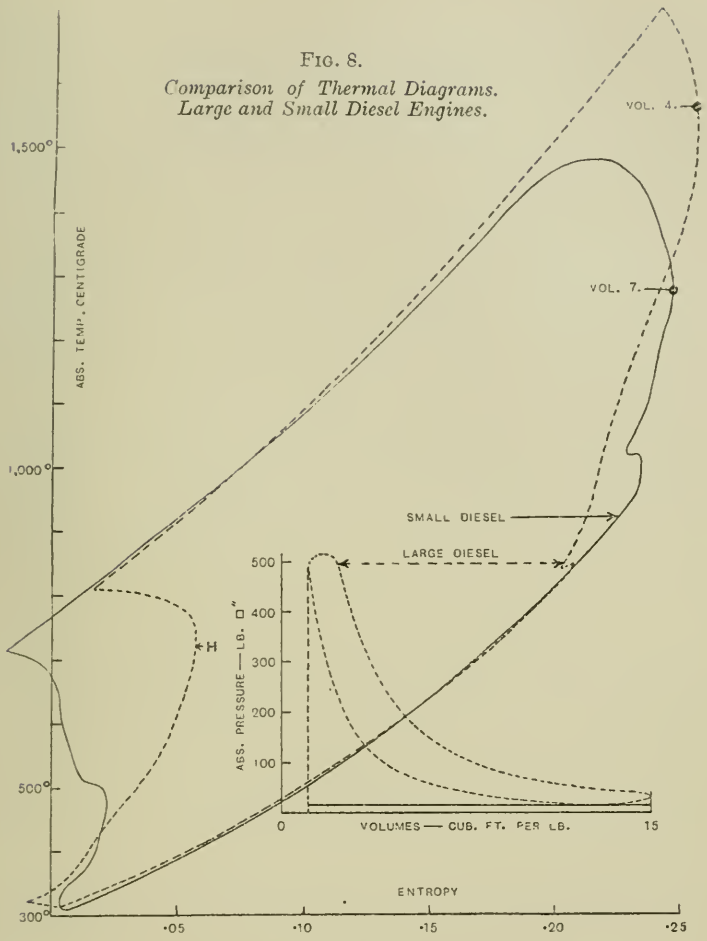


FIG. 8.
Comparison of Thermal Diagrams.
Large and Small Diesel Engines.

diagram * published by Mr. J. T. Milton. It is the average of a great many taken from the 4-stroke Diesel engines of the "Suecia," and is given by way of comparison with the diagram (also shown in Fig. 8) from the small Diesel engine already considered. The

* "The Marine Diesel Engine," Proceedings, Institution of Naval Architects, 2nd April, 1914, or *Engineering*, 10th April, 1914.

dimensions of the "Suecia" engines are not available, but they are rather smaller than those of the "Selandia," which has diameter of cylinder 20·8 inches, and stroke 28·7 inches, mean revolutions 138·5, and develops 174 i.h.p. per cylinder. The compression ratio in the "Suecia" engines is 13·5. Mr. Milton gives temperatures all round the cycle that are considerably higher than those shown in the thermal diagram, Fig. 8.

The cycle efficiency = 0·427. This is presumably for full load. The efficiency of the "Suecia" engines is not given by Mr. Milton, but the figure 0·427 is probably not much different from the actual indicated thermal efficiency. The ideal efficiency ratio was found to be 0·86.

The thermal diagrams give at once the temperatures at most points of the cycle. For the Crossley and the small Diesel engine the volumes are figured round the thermal diagram. If the crank positions are marked off, a good idea can be had of the rate of heat reception, by the gas, at different points whether arising from chemical cause or conduction.

Compression and Combustion.—Bearing in mind that in going round the diagram in the clockwise direction the gas receives heat when the movement is to the right and gives out heat when the direction is leftwards, the nature of compression, explosion and combustion can be ascertained; so also can be found the point in the compression stroke at which the gases have the same temperature as the average for the metal.

This temperature occurs at the point where the compression line is vertical, and has a value about 460° C. abs. (365° F.) for the Crossley engine, and about 480° C. abs. (403° F.) for the small Diesel. In the Crossley the corresponding volume is about 11 cubic feet per lb., and in the small Diesel is about 6½ at a point fully one half of the stroke. In both cases there is after-burning, but it is more pronounced in the Crossley engine.

Comparing now the small and the large Diesel engines, the compression lines show that the larger cylinder and piston are much hotter than the smaller; and the hot cylinder may account

for earlier and better combustion in the larger engine, as the expansion lines on the right would indicate. The average temperature of the cylinder and piston surface in the larger engine is seen from Fig. 8 to be approximately 750° C. abs. (889° F.) at the point H, which temperature is near the limit for the best lubricating oils. This indicates plainly the well known practical difficulty experienced in keeping cool the piston and internal surface of the cylinder of large Diesel engines.

The nature of the combustion is well illustrated in Fig. 8. In the larger Diesel combustion is nearly complete at point 4, equivalent to 0·36 of the stroke, and in the smaller Diesel combustion is almost completed at point 7, equivalent to 0·46 of the stroke. The thermal diagram should therefore be of considerable service in "tuning up" engines to give their best indicated results.

The reception of heat that the thermal diagram shows occurs in most cases at the end of the expansion curve may be due to the burning of the lubricating oil.

The Paper is illustrated by 8 Figs. in the letterpress.

[*See* Author's further explanatory Notes, page 352.]

Discussion.

Mr. J. C. M. MACLAGAN wrote that it was interesting to note, that, although the efficiency, as stated by the Author (page 343), was not seriously affected by a change in the suction pressure, the capacity of the engine might be considerably altered. A drop of 1 lb. in the pressure at the beginning of compression reduced the capacity of the engine by about 7 per cent.

(Mr. J. C. M. Maclagan.)

Referring to the dotted diagram reproduced on page 347, it was surprising to observe the very slow rate of change of temperature, and the sudden fall of entropy towards the latter end of the compression stroke. From the entropy-temperature diagram it would appear that the very high compression-pressures adopted in this type of Diesel engine were not entirely justified, as the latter part of the compression stroke approximated to an isothermal operation. Any reduction in the maximum compression-pressure would of course involve a corresponding reduction in the ratio of expansion, and therefore a loss of efficiency, but this would probably be more than balanced by the reduction in weight and first cost, and the increase in reliability.

With further reference to page 347, it was surprising to see that the combustion-line in the case of the large engine was steeper than in the case of the small engine, as the top of the indicator diagram for the large engine was flatter than that for the small engine. Speaking generally, the application of the entropy-temperature diagram should be of great interest to all concerned in the development of the internal-combustion engine, and the Author was to be congratulated on having explained its uses so clearly and fully. It was unfortunate, however, that the chart which no doubt must have been used in constructing the various examples had not been given in full, as without it, the value of the Paper was greatly reduced.

Captain THOMAS B. MORLEY wrote that, as he had taken a considerable interest in gas energy-diagrams for some time, he hoped he might be excused for referring almost exclusively to his own efforts in that direction. He regretted he had not access at the time of writing to previous publications, and if inaccuracies occurred in the following remarks they would be due to the fact that he was writing from memory.

Mr. Alexander's chart was an entropy-temperature diagram plotted with rectangular co-ordinates. Some years ago Professor Burstall published a gas energy-diagram * which had certain

* Proceedings, I.Mech.E., 1911, page 171.

advantages. In studying that diagram, it occurred to the present writer to try the effect of plotting pressures and volumes logarithmically, so getting rid of the convergence of the adiabatic lines on Professor Burstall's diagram, and thereby considerably simplifying its construction and use. The writer published a diagram on these lines in *Engineering*, vol. xcvi, page 445.

Subsequently Professor Stodola's chart in Zeitschrift des Vereines deutscher Ingenieure, 1913, Vol. 10, which Mr. Alexander had referred to in the footnote on page 333 of his Paper, was carefully studied, and a chart published by the writer in *Engineering* of 18th December 1914, and quoted by Mr. Alexander on page 334, was an attempt to combine the best points of all these gas-energy or entropy-temperature diagrams. It did not differ in principle from the entropy-temperature diagram. The points of difference were in arrangement only, and were essentially as follows :—(1) The temperature scale was logarithmic, so that volume and pressure lines became straight. The axis from which entropy values would be measured if required—it never was required to do so—was curved and raked at a convenient angle. In fact instead of plotting entropy from a straight vertical axis, an arbitrary position of the straight constant-pressure lines was chosen, and entropy values measured from these lines defined the adiabatic or constant entropy lines. The purpose of the arrangement was solely to produce a compact chart on which pressure, volume, or adiabatic lines should be in parallel sets, on which these lines should be straight, or if that was impossible should have only gradual curvature, and on which normal gas-engine diagrams should not have the expansion and compression operations represented by very short lines compared with the approximately constant volume or constant pressure operations.

Actual indicator diagrams could be transferred to this chart by reference to the pressure and volume scales as in Mr. Alexander's examples. The position of the lines of the transferred indicator diagram with reference to the adiabatic lines, etc., would represent the nature of the heat transferences in a similar manner, and the actual heat exchanges between specified points could be measured

(Captain T. B. Morley.)

by reference to the scales of internal energy and of "ART," which in combination gave also the "total energy" values. The writer's chart could also be readily adapted to suit different constants for different gas-mixtures. It was not possible to measure heat or work by measuring areas on this chart, but as the energy value at successive points could be read off from the scales, this was not a serious drawback.

The writer believed that, on the whole, his own chart had advantages over Mr. Alexander's, and in his opinion the chief value of Mr. Alexander's Paper lay in the examples of the application of such charts to the study of actual engine performances.

The AUTHOR, having returned to England during the period in which his Paper * was open for discussion, sent the attached further explanatory notes, also some corrections in the Paper itself which were now included.

The Paper has been considerably condensed from the original form in which it was written, and the chart has been given only in skeleton form as compared with the full chart submitted with the Paper. To some readers further proofs of several of the statements may be desirable, and these are appended.

The *transfer of heat* during any reversible change

$$= dQ = dE + APdv = T\,d\phi \quad . \quad . \quad . \quad . \quad (a),$$

= the area under the line, on the entropy-temperature diagram, corresponding to the process.

Hence, when the *pressure is constant*, AD, Fig. 2 (page 340), it follows from the definition of total heat that the transfer of heat is

$$\int dE + AP\,(v_d - v_a) = E_d - E_a + I_d - E_d - I_a + E_a$$

$$= I - I_a.$$

The work done $= AP\,(v_d - v_a) = I_d - E_d - I_a + E_a$.

* An Abstract of the Paper was printed in the Journal, and Advance Copies were available to members in November 1916.

When the *volume is constant*, BD, Fig. 2, it also follows from equation (α), that the heat transfer is $E_d - E_b$; and, from the same equation, the transfer of heat during the *constant temperature* process AC is seen to be $(\phi_c - \phi_a)\, T_{ac}$.

The work done during any reversible expansion such as DF, *Fig.* 2 :—

From equation (α), $APdv = dQ - dE$.

Therefore the area $QDFR = \int APdv = \int Td\phi + E_d - E$

$$= E_d - E_f + A^1 \quad . \quad . \quad . \quad (\beta),$$

where A^1 is the area on the entropy-temperature diagram, Fig. 3, between DF and the axis of entropy at zero temperature = the heat taken in during the expansion = area TDFS.

The work represented by the area GDFH, *Fig.* 2 :—

Since, from the definition of total heat,

$dI = dE + APdv + Avd P$, then it follows from (α) that

$dI = dQ + Avd P$.

$$\therefore \quad -\int Avd P = -\int dI + \int dQ.$$

Now $-\int Avd P$ = area GDFH in heat units.

Therefore, area $GDFH = I_d - I_f + A^1 \quad . \quad . \quad (\gamma)$.

Work done by the gas during adiabatic expansion, DC, *Fig.* 2 :—

From the equation for adiabatic expansion corresponding to equation (β) for polytropic expansion, and since there is no heat transfer the quantity corresponding to A^1 is zero, therefore the work done = area QDCM = $E_d - E_c$.

The work represented by the area $GDCH = I_d - I_c$. This is derived from equation for adiabatic expansion corresponding to (γ) for general expansion by putting A^1 equal to zero.

Work done during an isothermal expansion AC, *Fig.* 2 :—

From the equation (β) for general expansion, and since there is no change of internal energy in an isothermal expansion, the work done = area corresponding to $A^1 = T_{ac}\,(\phi_c - \phi_a)$ = area ACMN.

(The Author.)

Also, the work equivalent, in heat units, of the area GACH $= (\phi_c - \phi_a)\ T_{ac} + I_a - I_c$, which results from the equation for isothermal expansion corresponding to (γ) for general expansion by putting $(\phi_c - \phi_a)\ T_{ac}$ for A^1, this being seen to be correct by AC in Fig. 3 being a horizontal line at temperature T_{ac}.

Work in heat units represented by the area ABD, *Fig.* 2 :—

Area ADB = area GDQO − GANO − NABQ

$$= AP_d v_d - AP_a v_a - \text{area NABQ}$$

$$= I_d - E_d - I_a + E_a - (\phi_b - \phi_a)\ T_{ab}.$$

Also the work represented by the area KDC = area QDCM − area HCMO + area HKQO $= E_d - E_c - I_c + E_c + I_k - E_k = E_d - I_c + I_k - E_k$.

The index n_1 *of polytropic expansion*, DF, *Fig.* 2 :—

Referring to Fig. 3, it is seen from equations (12) and (13) (page 338) that at a given temperature

$$\phi_{a_1} - \phi_f = -R \log_e \frac{P_a}{P_f} = A_1F, \text{ and}$$

$$\phi_{b_1} - \phi_f = R \log_e \frac{v_{b1}}{v_f} = B_1F.$$

$$\therefore \frac{A_1F}{B_1F} = \frac{\log_e P_{a1} - \log_e P_f}{\log_e v - \log_e v_{b1}}.$$

But if the law $Pv^{n_1} = c$ represents the expansion, then

$P_d v_d^{n_1} = P_f v_f^{n_1}$, from which, since $P_{a1} = P_d$ and $v_{b_1} = v_d$, it follows that $P_{a1} v_{b1}^{n_1} = P_f v_f^{n_1}$. Therefore $\log_e P_{a1} - \log P_f = n_1 \times (\log_e v_f - \log_e v_{b1})$. Whence $n_1 = \frac{A_1F}{B_1F}$.

Similarly for the adiabatic expansion DC, the index is $n = \frac{AC}{BC}$. Now $\frac{AC}{BC}$ is approximately the ratio of the area under AD, Fig. 3, taken to the axis of entropy, to the area under BD, which areas are equal to respectively the heat taken in during the constant pressure process AD and the heat taken in during the constant volume process BD.

$$\therefore\ n = \frac{AC}{BC} = \frac{I_d - I_a}{E_d - E_b} = \frac{I_d - I_c}{E_d - E_c}.$$

The AUTHOR wrote, in reply to Mr. Maclagan, that it might be explained that the average direction of the *pv* combustion line was more downwards to the right for the small Diesel engine than for the large engine. This was the reason for the steeper Tϕ line of combustion for the large than for the small engine.

In reply to Captain Morley, although he claimed greater simplicity in use for his chart than for Professor Burstall's, the Author could not agree to this. The wide claims made by Captain Morley, and his "attempt to combine the best points of all these energy-diagrams" more or less ignored the requirements of a diagram that was to be applicable for the practical cycle and problem as apart from the theoretical, and that was to be of general utility rather than easy in construction. Although, as he said, actual indicator diagrams could be transferred to his chart, what then? He stated that heat exchanges between specified points could be measured by reference to scales of internal energy and of "ART." This could not be done for processes in general between the points, except by a comparatively laborious step-by-step method, due to the fact that areas on his diagram did not represent energy. It followed that the position of a process-line with reference to the adiabatic lines did not so well represent heat transferences as in the chart of the Paper. For the same reason the obtaining of efficiency of the practical cycle would be a lengthy proceeding with his chart.

The shortness of the constant volume and the constant pressure lines in the Author's chart was not a real objection, as these lines on the original chart (which measured about 20 inches by 28 inches) were about three times the length to which they were reduced in the Paper. Indeed, there was a positive advantage in having these lines as they were on the present chart, since the relative importance of the various heat transferences during the cycle was preserved.

For application to such purposes as—determining rates of heat transfer, placing a line which represented any expansion according

(The Author.)

to $pv = c$, finding the approximate index of expansion and compression in the practical cycle, working out problems in connexion with gas-turbines corresponding to those problems solved by the steam energy-diagram in connexion with steam-turbines, the chart described in the Paper was the more useful. It was also more in accordance with, and might be admitted to be more convenient than, the usual type of chart employed for other working substances, since it consisted of a ϕI chart superimposed on a ϕT chart while still retaining the simplicity of either and the advantages of both.

The Institution of Mechanical Engineers.

PROCEEDINGS.

May 1917.

An additional Ordinary General Meeting was held at The Institution of Civil Engineers, London, on Friday, 18th May 1917, at Six o'clock p.m.; Michael Longridge, Esq., *President*, in the Chair.

The Minutes of the previous Meeting were read and confirmed.

The President announced that the following ninety-five Candidates had been duly elected:—

MEMBERS.

Bannatyne, Andrew Walter,	Buenos Aires.
Deane, Henry James,	London.
Horrocks, Albert Edward,	Reddish.
Ince, David Daniel,	Kuala Lumpur.
Lyall, Alexander,	London.
Rothstein, Samuel,	London.
Watanabe, Jusuke,	Kobe, Japan.
Williams, Harry,	Glasgow.
Windeler, George Edward,	Stockport.

ASSOCIATE MEMBERS.

Aspar, Pestonjee Eduljee,	Bombay.
Barnes, Harold Noel Wykeham, 2nd Lieut. R.E.,	I.E.F.
Bates, Percy Vernon,	Blackburn.
Bentley, Hubert,	Halifax.
Beresford, Albert Ernest,	London.
Bergstrom, Eric Maurice,	London.
Bingham, James Herbert,	Birtley, Co. Durham.

Bolton, Charles John Howard,	Calcutta.
Brazier, Clifford Claude Hendy, Lieut. I.A.R.,	Lahore.
Brodie, Edward David, Lieut. A.S.C.,	B.E.F.
Bugg, Ernest Archibald,	London.
Carter, Stanley Cyril, Lieut. R.E. (T.),	B.E.F.
Cook, Frederick William,	London.
Crawley, Herbert George,	London.
Dalkin, George Robert,	Ottawa.
Davis, James Archibald,	Dublin.
Ellson, Horace Stanley,	Rugby.
English, John,	Farnborough, Hants.
Fielden, John Albert,	Manchester.
Fleming, Samuel Henry, Captain R.I.F.	B.E.F.
Ghosh, Surendranath,	Sakchi, India.
Glover, Ronald Brown, Lieut. R.E. (T.),	Manchester.
Goodier, Robert,	Manchester.
Green, Victor Edward,	Birmingham.
Hamilton, Charles Joseph,	Hull.
Hammond, Ralph,	Bolton.
Hampton, William Frederick,	Melbourne.
Harley, George Bauchop,	Kuala Lumpur.
Harrison, Frederick Charles,	Chester.
Head, George Burges Digby,	London.
Healey, William,	Woolwich.
Herbert, Edward Dave Asher, Lieut. R.G.A.	Erith.
Hide, Walter Daniel,	London.
Hill, Arthur Trenchard,	Walton-on-Thames.
Hitchcock, Geoffrey Edmund William, Lieut. R.E.,	London.
Hughes, Percy Henry, Lieut. A.S.C.,	B.E.F.
Jeffery, Samuel James,	London.
Johnson, Gilbert King, 2nd Lieut. R.F.C.,	Ramsgate.
Kitchen, Albert,	Halifax.
Kniveton, Herbert,	Kingston-on-Thames.
Koster, Frederick William, 2nd Lieut. R.E.	Sandwich.
Lambert, Charles William, Eng. Lieut. R.N.	H.M.S. "Undaunted.'
Latham, Percy Barkley,	H.M. Sub. E 51.
Lockley, William Hugh,	Madras.
McBryde, Anthony Henderson,	Newcastle-on-Tyne.
Male, Edward,	Llanelly.
Mehta, Framroz Dhanjishaw,	Bombay.
Mitchell, James Boon,	London.

Moore, John Leslie Mackenzie, Capt. R.E.,	B.E.F.
Naylor, Thomas Martin, Eng. Lieut. R.N.,	Birkenhead.
Pateman, Arthur, ...	Leicester.
Penny, Frederick Gaston, . . .	Manchester.
Plowman, Herbert Samuel, . . .	Sheffield.
Preston, Alfred Samuel, Captain A.S.C., .	B.E.F.
Rendell, Samuel Hubert, . . .	Lincoln.
Rogers, William, ...	Stroud, Glos.
Roskilly, William Willcock, . . .	Leeds.
Ryley, Walter, ...	London.
Smith, Randall Bonner, Eng. Sub-Lieut. R.N.R., ...	H.M.S. "Eridge."
Spencer, John Arthur, Lieut., A.O.D.. .	Pembroke Dock.
Stevens, Herbert Edward, . . .	Singapore.
Stevenson, Robert, Captain A.O.D., . .	B.E.F.
Stone, Ernest George, ...	London.
Surman, Edward John, ...	Hong Kong.
Taylor, Reginald Edgar, . . .	Burnt Mill.
Thomas, William Norman, Lieut. R.E., .	B.E.F.
Townend, Harry, ...	Manchester.
Townsend, Charles Eric, Lieut. R.F.A. .	Boyton.
Vaughan, William Henry, . . .	Liverpool.
Vickers, Harold, ...	Nottingham.
Wainwright, Thomas Robert Aiken, .	Sheffield.
Walker, Charles Hugh, Lieut. R.N.V.R.,	H.M.S. "Furious."
Walker, John Robert, ...	Tewkesbury.
Waterhouse, David, ...	Leeds.
Webster, John Herbert, . . .	Carrickfergus.
Williamson, Robert Charles, . . .	Southampton.
Willshere, Egbert, ...	Tongham, Surrey.
Woolley, Arthur, ...	Howrah.

ASSOCIATE.

Goold, Lewis William, ...	Birmingham.

GRADUATES.

Anderson, David Stirling, . . .	Glasgow.
Couchman, Richard Seymour, . . .	London.
Edwards, Christopher Frederick, . .	Bushey Heath, Herts.
Greening, John Kenneth, . . .	St. George's, Salop.
Griffiths, Edward, ...	Newcastle-on-Tyne.
Hedley, Reginald, ...	Sunderland.
Mehrez, Ahmed Mohammed, . . .	Birmingham.

The PRESIDENT announced that the following six Transferences had been made by the Council:—

Associate Members to Members.

CRUICKSHANK, ANDREW, ...	London.
DIVECHA, TOOLJARAM MOOLJI, . . .	Ahmedabad.
HUNTER, JOSEPH MOORE, ...	London.
JANSON, ROWLAND, Capt. A.S.C. (M.T.), .	London.
NEWMAN, WILLIAM HENRY, . . .	London.
WILLIS, GEORGE OSWALD BELL, . . .	Sheffield.

The following Paper was read and discussed:—

"Notes on the Construction of Turbine-Pumps"; by ALAN E. L. CHORLTON, of Lincoln, *Member*.

The PRESIDENT asked the members to pass a cordial vote of thanks to The Institution of Civil Engineers, for lending their rooms to the Institution for the purpose of holding the Meetings, and also for the hospitality which had been extended to them.

The Resolution of Thanks was carried by acclamation.

The Meeting terminated at Half-past Seven o'clock. The attendance was 60 Members and 38 Visitors.

NOTES ON THE CONSTRUCTION OF TURBINE-PUMPS.

By ALAN E. L. CHORLTON, of Lincoln, *Member.*

The turbine has become so highly successful as a pumping engine for duties of all description, that no apology is needed from the Author for again returning to the subject and offering a few practical notes on the design and construction of its detailed parts.

In passing, it is interesting to refer on the theoretical side, to the early French works of M. Poncelet (1838),* and M. Combes (1843),† on reaction wheels generally; also to the two Papers on the mathematical principles involved in the centrifugal pump by Mr. Andrew J. Robertson in the Proceedings of this Institution, 1852 and 1853, culminating in the classic work of Dr. Unwin in the Proceedings of the Institution of Civil Engineers 1877–1878. Early practical applications of accepted principles have been given by such work as that of Mr. Appold, 1848–1851, with his well-

* "Mémoire sur les turbines de M. Fourneyron," read at "l'Academie des Sciences," 6th August 1838.

† "Récherches Théoriques et Éxperimentales sur les roues a Réaction ou a Tuyaux," by M. Chas. Combes, Paris, 1843.

In which M. Combes refers to his own experiments and also to the turbines of several other makers and to the centrifugal pump of M. Demours 1732.

known pump, Professor James Thomson,* with his vortex or whirl-chamber pumps put down in 1852 to 1858, and by Mr. R. C. Parsons in experimental work described in a Paper read before the Institution of Civil Engineers in 1877. Latterly practical development has been furthered by experimental work on the efficiency of centrifugal pumps by Dr. Stanton, and published in the Proceedings of this Institution in 1903, by the experiments on the fluid friction of rotating disks carried out by Dr. Unwin and described in the Proceedings of the Institution of Civil Engineers, Volume LXXX, and more recently by the very valuable work of Professor Gibson,† extending over wide ground both directly and indirectly affecting the centrifugal pump. These are milestones in the history of the evolution of the centrifugal pump, yet the wide extent of its successful practical application in Turbine form is due to the overcoming of practical difficulties both mechanical and hydraulic. This work has only been accomplished after prolonged experience, and success has only been reached after tedious and careful study and improvement of small details.

The object of the present Paper is to describe and trace the

* Thomson whirl-chamber pump put down at Low Lodge Mill near Belfast in 1852. Another designed for Land Drainage in Jamaica in 1853 and made in Glasgow by Messrs. W. and A. M'Onie and Co. Also a third built by Jas. Ewing and Co., of Glasgow, for Demerara in 1858.

See Papers read before Institution of Engineers in Scotland, vol. 1, page 90, and "on a Centrifugal Pump with Exterior Whirlpool constructed for Draining Land," read 27th October 1858.

† "On the Flow of Water through pipes and passages having converging or diverging Boundaries," Proceedings, Roy. Soc. A. vol. lxxxiii, 1910, page 366.

"On the Resistance to Rotation of disks in water at high speeds," Proceedings, Inst. C.E. vol. clxxix, Session 1909–1910, Part 1.

"On the resistance to flow of water through pipes or passages having diverging boundaries," Trans. Roy. Soc. Edinburgh, vol. xlviii, Part 1 (No. 5), 1911.

"The loss of energy at Oblique Impact of Two Confined Streams of Water," Trans. Roy. Soc. Edinburgh, vol. xlviii, Part IV (No. 28), 1912.

"The design of Volute Chambers and of Guide-Passages for Centrifugal-Pumps," Proceedings, I.Mech.E., 1913, page 519.

development of some of the methods of successfully overcoming the difficulties referred to, and to draw attention to points in design which are the result of the Author's experience extending back to the infancy of the application of the Turbine-Pump Principle.

It will be convenient to consider the subject under the headings of the various component parts of a pump. These are :—

I. The Stator, which consists of (I *a*) the casing or housing and (I *b*) the guide-vanes or appliance for converting velocity energy into pressure energy.

II. The Impeller (considered separately).

III. The Balancing Appliance—hydraulic or mechanical.

IV. The Rotor, considered as a whole, and including the spindle with its protecting sleeves, impellers, and in most cases the balancing appliance.

V. The Bed, and other details, bearings, stuffing-boxes.

Of these components III, IV, and V, broadly speaking, are the factors governing reliability and resistance to wear, and I *b*, and II are those determining the efficiency.

The Author proposes to deal at greater length with III and IV, for the reason that they have probably been less discussed than the others, also their practical importance is of the greatest moment, and he cannot deal with the whole subject adequately within the confines of a Paper written during the present time.

The chief points only of items I, II and V will be briefly reviewed :—

I. Casing.—The individual impellers of a turbine-pump revolve in chambers or cells containing the outward flow guide-passages, and the return conduits for the water; these chambers may be part of a whole in which the outer body is cast in one piece, or, an aggregation of a number of distinct cells without any outside envelope.

There are therefore two main types :—

(1) The Osborne Reynolds or divided type, sometimes called the "ring-type."

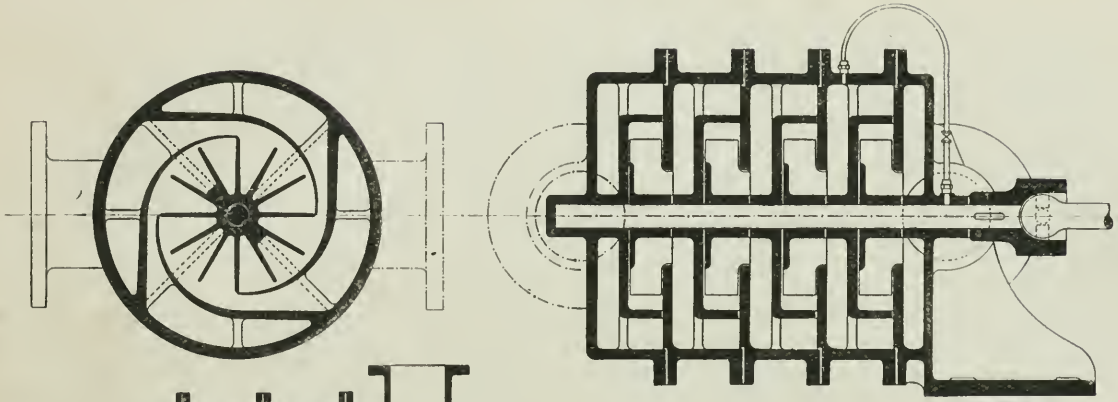


Fig. 1 *a*.
(Osborne Reynolds.)

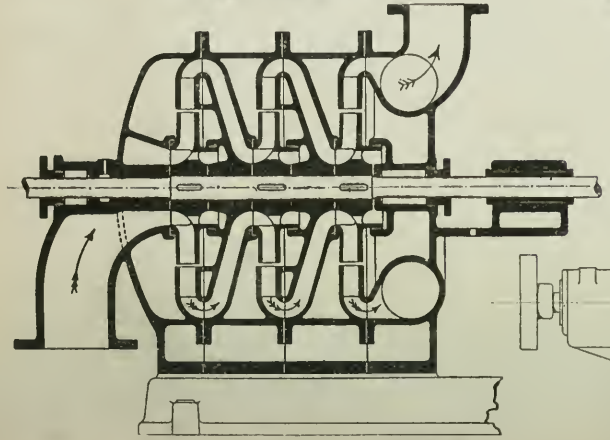


Fig. 1 *b*.—"*Ring-type*" *Construction.*

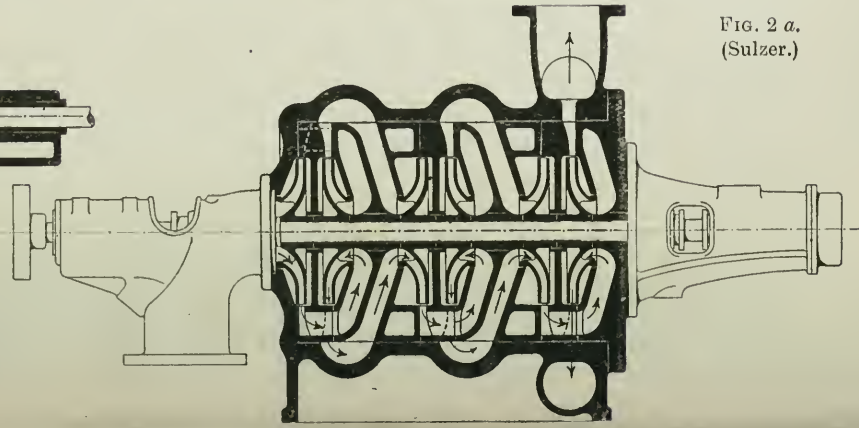


Fig. 2 *a*.
(Sulzer.)

TYPES OF CASING.

FIG. 2 *b*.—*Continental Pump. "Cylindrical-casing" Construction.*

FIG. 4.—*Combination of "Cylindrical" and "Ring" Features.*

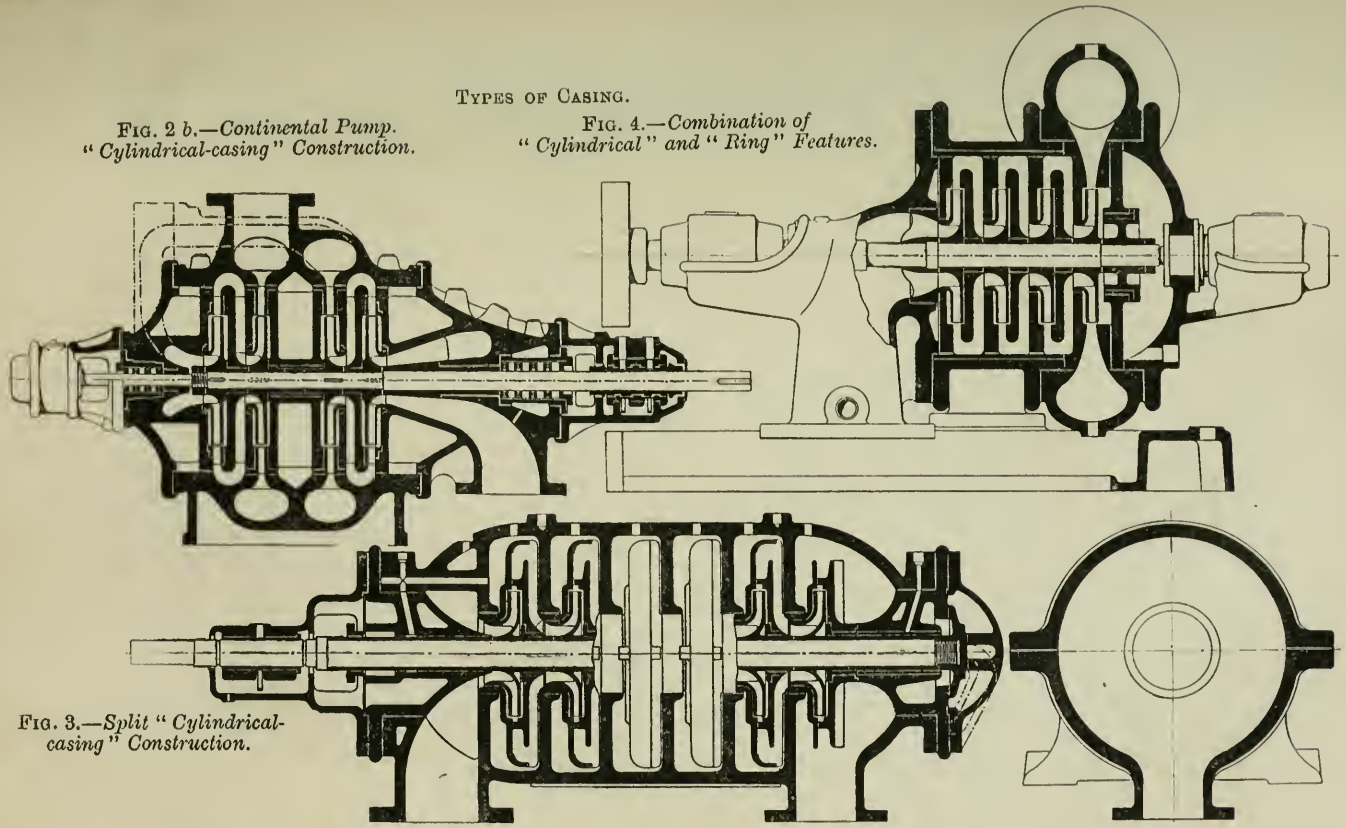


FIG. 3.—*Split "Cylindrical-casing" Construction.*

(2) The Sulzer, integral or one-piece type, sometimes called the "cylindrical" type. Recent American practice provides a variation of this type with the housing in halves divided on the horizontal centre line.

The two main types are illustrated diagrammatically by Fig. 1 (*a* and *b*) and Fig. 2 (*a* and *b*) with variations in Figs. 3 and 4 (page 365).

The Osborne Reynolds pump and its evolution was dealt with by the Author in collaboration with Dr. E. Hopkinson, in some detail in a Paper read before the Institution in January 1912. This type was followed at a later date by the integral one of Sulzer, which had, for a period, a considerable vogue. An examination of the present-day practice of various makers will prove, however, that the ring-type has ultimately proved the preferred one. The Reynolds pump * has always used a separate cell for each impeller, and it is therefore correct to say that the ring-type of pump owes its inception to this country.

The Continental form of the series turbine-pump is due to Sulzer (first pump 1896), and in this type a monoblock housing was used for all the chambers or cells, the guide-vanes being inserted from the end.

Before dealing with either type or variations arising out of them, it is advantageous to consider the essential functions of the chamber or housing of a turbine-pump. Primarily, each cell consists of (1) the outward flow guide-passages in which the kinetic energy of discharge from the turbine-impeller is converted into static head, and (2) the return water-passages back to the centre for conduction to the next impeller; a complete housing is a collection of such cells.

Obviously, in the design, commercial considerations must have a material guidance on theoretical claims. Against the requirements for best theoretical conversion of kinetic energy must be matched

* First patented No. 724—1875. It is interesting to note that the drawing in this specification shows a pump which is practically Professor Jas. Thomson's turbine reversed.

the allowable limits of dimensions conformable with commercial possibilities, and, in the interests of efficiency, special attention must be paid to the arrangement and dimensions of the divergent channels or guide-passages. As is well known, the form for guide-passages is represented diagrammatically by Fig. 5 (page 368), and this passage must be disposed in some form to lie conveniently in the desired casing. The general character will be either a simple outward flow type in one plane, Fig. 6, or a mixed type outward and axial in two planes, *see* Fig. 7 (*a* and *b*) and Fig. 8.

The divergent angle of guide-vane, Fig. 5, for best efficiency was shown by Professor Gibson * to be 10° to 11°. In many cases such a small divergent angle leads to a large overall diameter guide-vane in order to give a sufficiently reduced speed of water to permit reversing its radial direction, and the result is a very heavy casing in consequence. For this reason divergent angles of 15° are commonly found in practice. Evidently a more efficient pump will sometime be heavier and more expensive than one less efficient, and commercial considerations must provide the final deciding factor between efficiency and weight. There is, of course, a school of design which believes in dealing with a proportion of the velocity conversion in the wheel itself, thus leaving less to be dealt with in the guide-passage; the extent, however, to which this method can be used for weight saving is very small, if any.

The various assemblies of passages may clearly be grouped into:—

(A) Tangential and radial with return radial (*see* Fig. 6 and Fig. 1, *b*).

(B) Tangential and spiral (*see* Fig. 7 (*a* and *b*) and Fig. 8).†

(C) Combinations (*see* Figs. 9, 10 and 11).

The Osborne Reynolds pump (1887 and 1875 type) employed an early form of the (B) assembly, and has adhered to this type up to

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* Divergent angles have been most thoroughly investigated by Professor Gibson, *see* Proceedings, Roy. Soc. A. vol. 83, 1910, and Trans. Roy. Soc. of Edinburgh, vol. xlviii, Part 1, No. 5, 1911, etc.

† *See* Hopkinson and Chorlton, Patent No. 8855—1904.

FIG. 5.—*Form of Guide-Passage. Throat only.*

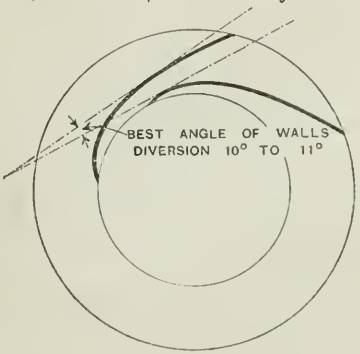
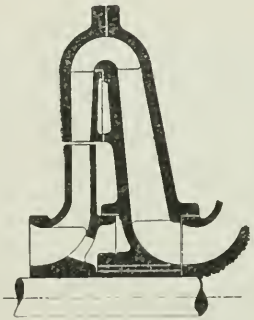


FIG. 6.—"*Stage*" *or* "*Cell.*" *Flow tangential and radial, return radial.*



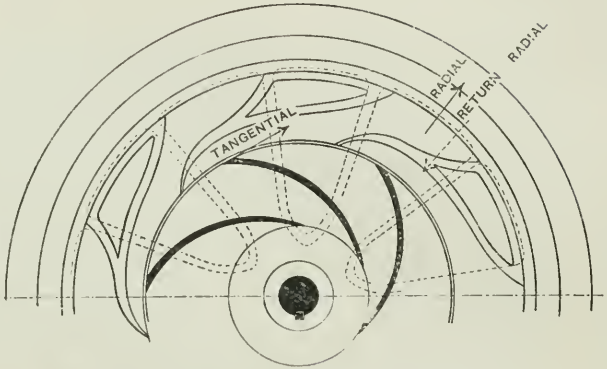
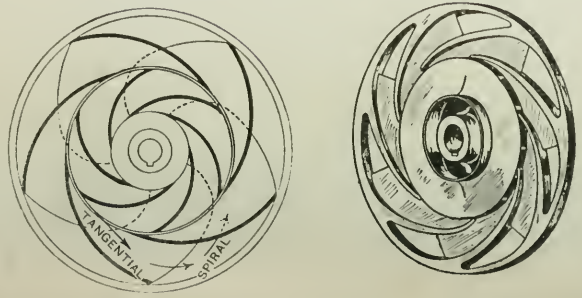


FIG. 8.—*Single "Cell," single passage only.*
(Worthington Pump Co.)

FIG. 7.—*Flow tangential and spiral.*
a. Flat. *b. Perspective.*



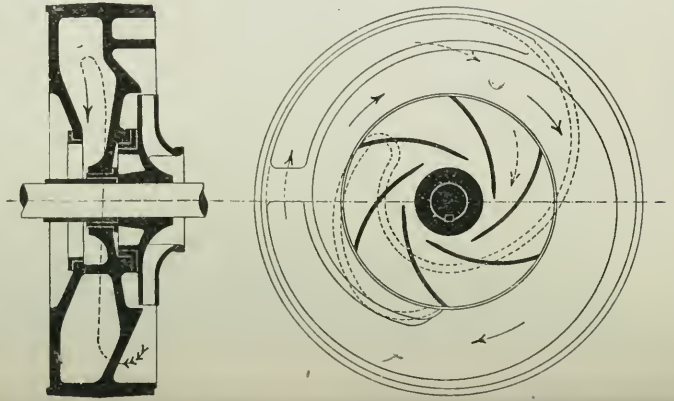


Fig. 9.
(Marchand.)

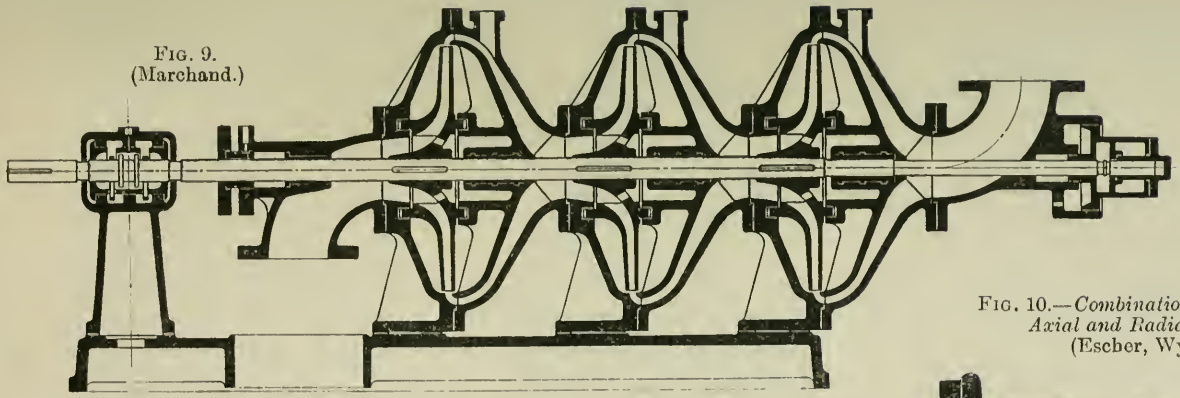


Fig. 10.—*Combination of Tangential, Axial and Radial Flow.*
(Escher, Wyss.)

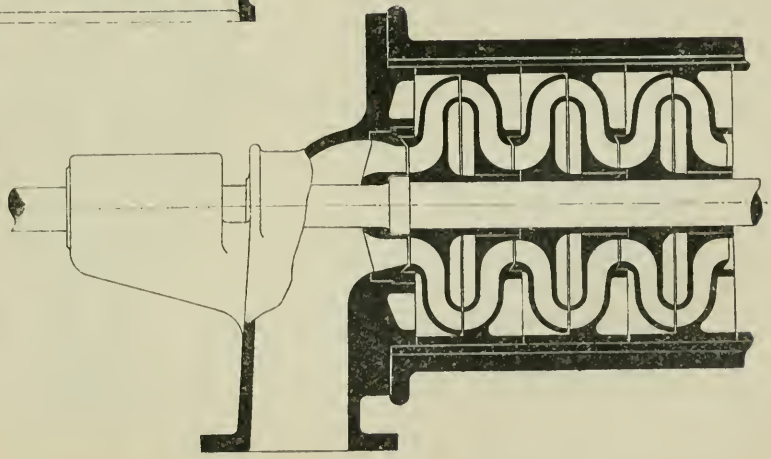
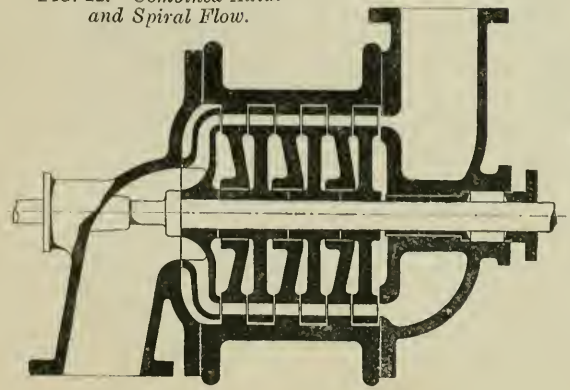


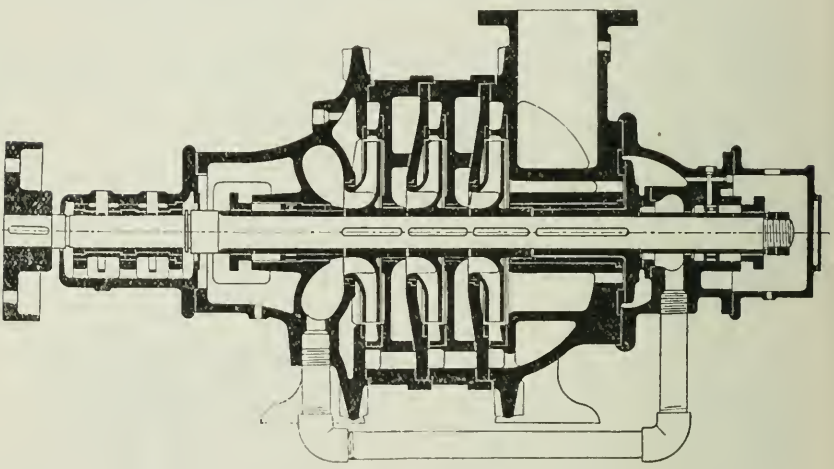
Fig. 11.—*Combined Axial and Spiral Flow.*



the present day, various improvements being embodied from time to time, in some of which the Author was concerned. On the Continent Messrs. Sulzer introduced in 1896 design (A), and the Author believes they have made little departure from the type beyond a considerable simplification of their early arrangement of passages, Fig. 2 *a* (page 364).

Speaking generally, combination designs (C) are not so efficient as the simpler types (A) and (B), owing probably to the hydraulic loss through changing the radial direction of the water at high speed.

FIG. 12.—*Modern Form of Turbine-Pump.* (Mather and Platt.)



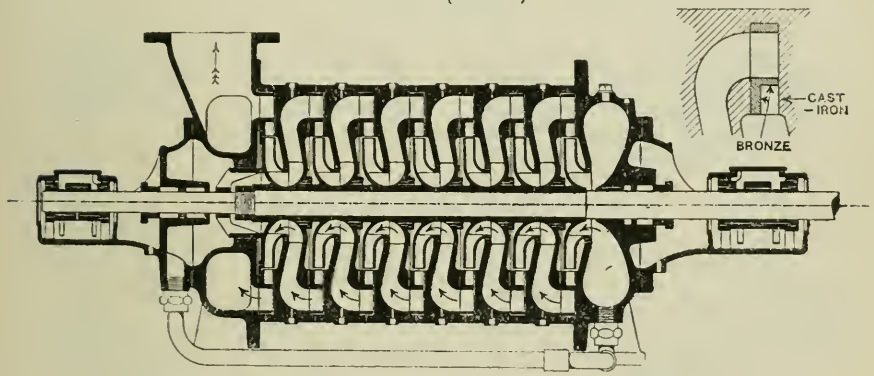
From a works construction point of view, "ring casings," Fig. 1 (*a* and *b*), are the most economical, and in practice give high efficiency. In the form similar to that shown in Fig. 12, the Author some years ago was able to mould and cast ring-casings without cores, machine moulding being adopted, and the cost per chamber coming out at a very low rate.

For "cylindrical" casings, Fig. 2 (*a* and *b*), a complete pattern is required for each size and variation in number of chambers. Its accessibility, however, and ease of dismantling is sometimes considered to be greater than with the "ring" type, though this is

to a certain extent, a matter of opinion. A great drawback to casings containing separate cells is that, on account of the sliding fit between the intermediate pieces and casing, an unknown amount of leakage constantly takes place between the cells. With a "ring" type of pump, leakage is instantly detected and can be remedied. As a commercial proposition the Author unhesitatingly favours the divided or "ring" type of casing, and when properly carried out, has met no difficulty with it in practice.

Before leaving this part of the subject, a word about the finish of guide-passage surfaces is necessary. For best efficiency the throat of the passage at least, if not the entire passage, should be

FIG. 13.—(Farcot.)



gun-metal or bronze, as iron does not preserve a sufficiently good surface for high velocity conditions. Common practice is to provide only three sides of the guide-passage in bronze, Fig. 13, but this can only be defended on grounds of cheaper first cost. For best results a bronze-plate should be provided to box-in the passage, the plate being attached to the guide-vane casting or dowelled to the casing. Guide-vanes are sometimes cast completely boxed in, and this method necessitates hand-finishing of the passage by file and scraper; open vanes, however, lend themselves better to cleaning out and accurately finishing either by hand or by machining. A good smooth surface is essential for the best results and will always justify the increased cost.

The method adopted of securing guide-vanes from rotation and vibration must be a thoroughly sound one or trouble will result.

II. IMPELLERS.—Impellers are either :—

(1) Single entrant, or

(2) Double entrant.

The first is almost universally in use for multicellular pumps, and the second almost exclusively for single-chamber pumps ; it is only proposed to deal with the single entrant form.

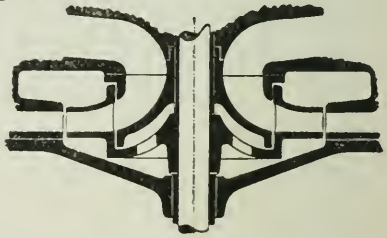
Multicellular pumps use the single-eye wheel in three forms :—

FIGS. 14 and 15.—*Impellers, Unbalanced.*

Unequal side area.
One rubbing shoulder.

Equal side areas.
Two rubbing shoulders.

FIGS. 16 and 17.—*Impellers, Balanced.*



(*a*) Unbalanced, unequal side area, one rubbing shoulder, Fig. 14.

(*b*) Unbalanced, equal side areas, two rubbing shoulders, Fig. 15.*

(*c*) Balanced—on paper, Figs. 16 and 17.

There are other types, mixed flow, radial and axial, etc., but these are not used to a sufficient extent to be worth including in this Paper.

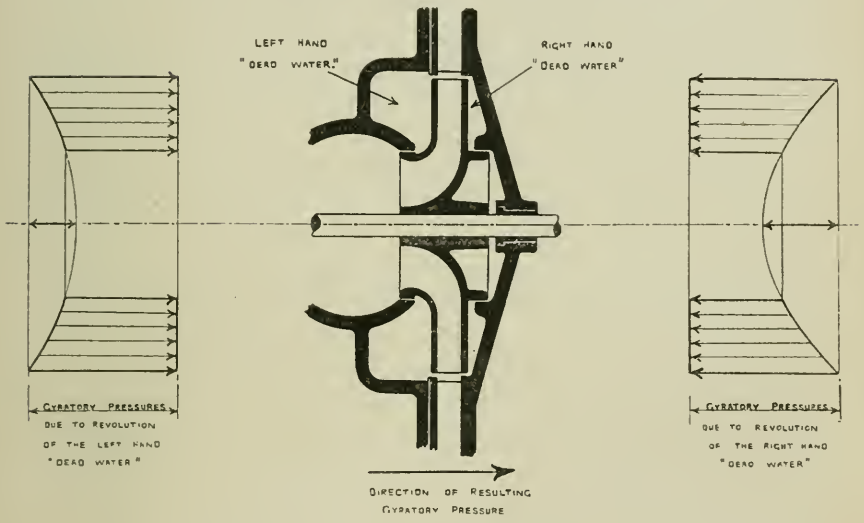
* *See* also Hopkinson and Chorlton Patent, No. 8855—1904.

Type (*a*) may be said to be the one now generally used, that is, the preferable design.

Type (*b*) has certain advantages for machining, etc., but probably requires a stiffer shaft. Also, having no central supports, it tends to rotate the suction water of the next impeller and may be attended by greater stage leakage due to the increased annular running clearance.

Type (*c*) has the *paper* advantage of being in balance; actually it is a poor approximation to a balance. Disturbing

Fig. 18.—*Effects of Gyratory Pressures on End Thrust.*



factors are set up by: differences in side pressure on the impeller due to differences in volume and surface-form of the water contained on the two sides of the impeller, Fig. 18; differences of quantitative leakage through the two shoulders; and high-pressure leakage *into* one side of the impeller from the stage above, and leakage *from* the other side of the impeller to next low pressure stage below. Therefore, in practice, it is necessary to provide an additional end balancing device

of the hydraulic type, or a mechanically positioning fitting such as a thrust-collar or ball-bearing; a method, especially for mine usage, not to be recommended.

The internal design of all impellers is governed by the same controlling features:—

(1) The entrance or inlet angle of vane.
(2) The delivery or exit angle of vane.

The entire design must, while based on these considerations, consult the convenience of the workshop to the utmost degree possible without departing from required dimensional accuracy. The standing difficulty with turbine-pumps, from the manufacturing point of view, is their constant variation to meet the infinite number of conditions of varying head, speed, and quantity encountered in practice. Whatever efforts are made, it seems impossible to keep to a small number of standard impellers if the highest efficiency is to be reasonably well reached each time. Efficiency, it should be noted, is really the prime factor in design and not apparent first cost, for the pump is very often driven by an electric motor of greater value. If, therefore, by the use of a pump of higher efficiency, a reduction is effected in the necessary power and size of motor, the combination will generally come out cheaper.

The maker who elects to change his impellers and diffusers to suit the demands of the inquiries as they come in, will obtain a higher percentage of orders than one who has standardized his, even though the latter may have reduced costs by making in large quantity. In considering if in any way it is possible to meet the designer's requirements without losing all the advantages of repetition manufacture, we may first take *the inlet angle*. This depends on the resultant of the (supposed) radial flow of the entering water and the peripheral speed of the inlet tip itself, and it usually varies between 15° and 30°. If we select to use always 15° we might suffer in some cases to the extent of 3 per cent. or 4 per cent. If we take two sizes, 15° and 25°, and make a liberal provision of inlet width, or, as some designers phrase it, make an adequate allowance for "weir coefficient" at

entrance, we shall only drop perhaps 1 per cent. in exceptional cases; so it seems possible to do something in the way of standardizing the inlet angle.

One might refer, in passing, to some of the current ideas concerning the inlet angle and the condition of the water at entry to the impeller. Some designers have maintained that very great accuracy of the inlet angle is vital to high efficiency, in spite of the fact that in the ordinary unobstructed eye it is impossible to say what the absolute velocity and direction of the incoming water is, and therefore, to estimate precisely what the correct inlet angle should be is impossible. Further, the behaviour of the incoming water varies with every rate of flow, and the only way to foretell its condition would be to insert inlet guide-vanes; this, however, in such experiments as the Author is aware of, has proved an objectionable practice and only introduces further losses in the pump. Another school holds that it is an advantage purposely to introduce a forward whirl in the water at the eye with the object of helping the water into the impeller. If such an initial whirl is used, the work necessary to create it is necessarily done by the pump itself, and it is, therefore, questionable if this does not entirely overweigh any possible gain.

The discharge angle depends on the resultant of the peripheral speed of the wheel and the radial out-flow, the variation of which is so great it seems impossible to devise any standardization. To meet quantity variation two widths of impeller may be used, and to meet required speeds of revolution it is usual to allow a small percentage variation in the impeller diameter to suit special demands; this latter, however, is conveniently done without pattern or casting alterations.

The foregoing considerations lead to the conclusion that nothing less than a special core-box will be required for each case, and it is quite usual to meet the difficulty in this way. Evidently, however, a most valuable appliance would be a special form of core-box, which in itself was more of a standard and could be adopted for manufacture in quantity; this result might be effected by fitting in vanes of a flexible nature, so that the necessary alteration and

adjustment would not be great, and the whole outfit would come out cheaper than if a pattern-maker had to build a fresh box and fittings for each new demand. As the accuracy and smoothness of the impeller is of great importance, the Author has always looked to machine moulding, and to a special machine like a wheel-moulding machine, for the purpose; he has not, however, as yet, arrived at the final design for such.

The surfaces of the impeller, both inside and out, play an important part in the efficiency of a turbine-pump. The smoother the outside surface the less the power lost in disk friction, and the less the power wasted in revolving idle "dead-water."* An interesting point to note is that the greater the speed of revolution of the "dead-water" (that is, the greater the power absorbed in this way), the less the leakage from the periphery of the impeller; but a little consideration will show that greater overall economy is gained by reducing the wasted power in idle revolution to a minimum. As regards the internal smoothness of the passages of the impeller, it is usual practice to clean up the surfaces as well as possible with file and scraper. Impellers have been built up with one loose side so as to permit of machining or more effectively cleaning the interior; and it is evident that with individual impellers producing high heads the results would well repay the extra cost. Objections to this practice are the difficulty of making attachment of the two parts and the extra weight necessary to provide attachments.

III. BALANCING APPLIANCE.—The experienced designer knows, from practical knowledge, that even if a group of impellers, or even a single impeller, is theoretically balanced by equivalent areas subject to pressure, it will not in actual practice be free from all end-thrust. Apart from variable leakage at the two sides of an impeller, which, by the way, is the principal cause of end-thrust, we often have variable side surface both of the impeller and the cell chamber. If the conditions of capacity or surface of the two clearance-chambers vary, the resulting pressures will vary and an

* *See* Professor Gibson's Disk Friction Experiments, already referred to.

axial thrust is set up. Professor Gibson has investigated the effects on efficiency (power lost) due to varying the side clearance of the impeller, and he took readings of circumferential pressures set up by revolving disks, but he apparently did not plot out the resultant effect in end-pressure. This interesting problem is discussed in a Paper by F. Zur. Nedden,* where use is made of the results from both Professor Gibson's and Professor Unwin's revolving disk experiments. The general effect of the gyratory pressures set up is indicated in Fig. 18 (page 373). It is seen that the speed of the revolving "dead" water at the two impeller sides sets up pressure in opposition to the leakage pressure from the impeller-tip, as already pointed out; the faster the water revolves the greater the resistance to leakage, and the less the resultant pressure due to that leakage. It can readily be seen, therefore, that the effect of the chamber at one side of a wheel having greater capacity than the other, or having more obstructions in the way of ribs, pockets, or exceptional roughness, etc., plays some part in the determination of end-thrust.

All the earlier turbine-pumps employed some form of thrust-bearing, either of the collar or the ball type, to keep the impellers in correct alignment with the guide-passages, and it was not until much trouble had been experienced with these that hydraulic control was ultimately adopted. Experience showed that much higher end-pressures were set up than were ever anticipated, but for a period mechanical devices were persisted in, improvements being made attempting to withstand the excessive loads, and scant notice, it would appear, being taken of water-turbine practice where for many years it had been the custom to relieve axial thrust hydraulically. Fig. 19 (page 378) illustrates an hydraulic balancing device† in use on Francis turbines, and which, if properly proportioned, is automatic, and thus embodies all the essential points of many present turbine-pump balancers. One should bear in mind that the relation between the calculable axial thrust and the

* Am. Soc. Mech. Engineers, September 1915.

† Proceedings, Inst. C.E., 1909, vol. clxxvi, page 317.

thrust realized in a water-turbine is much closer than in a centrifugal pump of the multi-stage type, the reason of this being that the disturbing factor in a high-lift pump is the leakage from stage to stage.

The well-known application of turbine-pumps to high lifts by Messrs. Sulzer at Horcajo Mines in 1898, was carried out with back-to-back impellers, and a ball thrust-bearing was provided to take the end-thrust (in one direction only), which is inseparable

FIG. 19.—*Hydraulic Axial Balancing Device.* (Chalmers.)

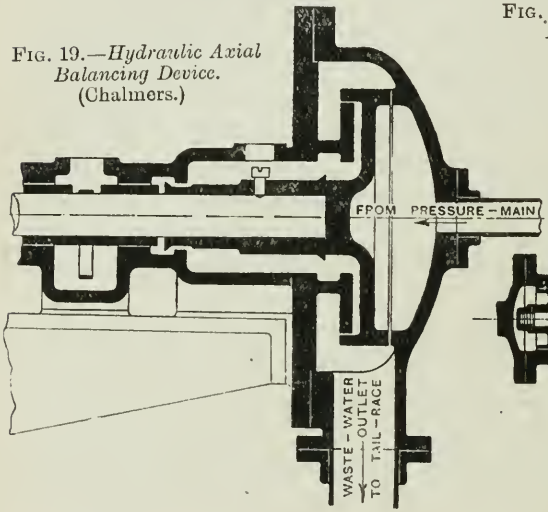
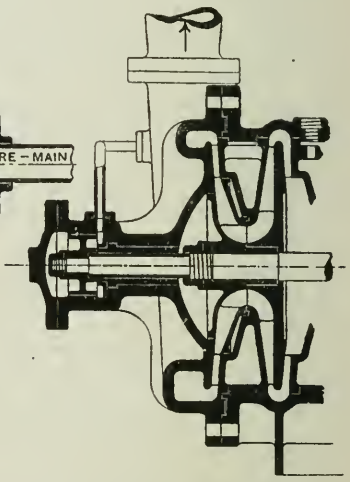


FIG. 20.—*Pump* (Rateau) *showing Hydraulic Compensator.*



from this arrangement of impellers. In 1901, Professor Rateau was manufacturing pumps with end-thrust approximately eliminated hydraulically by his well known method of shroud reduction, and provided with a balancing piston, Fig. 20.* This method was not automatic, and the pressure on the piston could only be adjusted by means of a hand-operated throttle-valve. However, this was the first step, and the automatic control of the necessary pressure on the balancing device and determined by the end movement of the spindle,

* Bulletin de la Société d'Encouragement pour l'Industrie Nationale (Paris), 31st December 1901, page 729, and British Patent, No. 18,106—1901.

followed as a matter of course. The development of the differential type of balancer carried out under the Author's direction,* extended over several years, progressing step by step from a hand-adjusted needle-valve, Fig. 21, regulated to produce the required balancing pressure, to a mechanically-operated needle-valve, Fig. 22, actuated by the axial movement of the spindle, and then through several forms of rotating throttle-valve disposed on the spindle itself and actuated by the same means. Further reference to the final balancing appliance (diagrammatically shown in Fig. 23) will be made shortly.

Fig. 21.—*Hand-adjusted Balancer.*

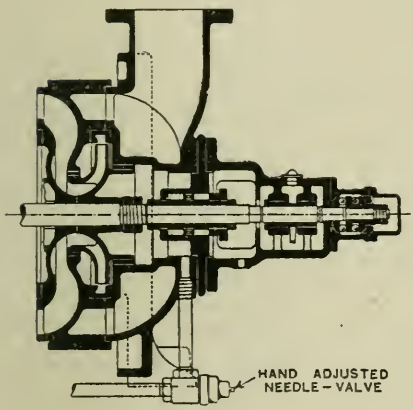
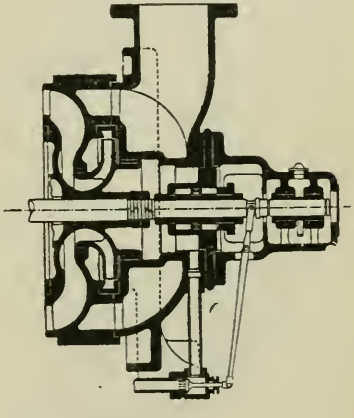


Fig. 22.—*Mechanically adjusted Balancer.*



In 1906 Messrs. Sulzer brought out their device for relieving thrust shown in Fig. 24, and the principle used has been followed by many other makers and introduced with many variations. The action of the Sulzer device is simple; it comprises a single plate with a throttling device, through which the pressure water has access to the plate. The area of the plate is sufficient when acted on by a somewhat throttled delivery-pressure to overcome the tendency of the rotor to travel in the opposite direction. As the pressure gradually rises before the plate, its increased power carries

* *Engineering*, 22nd August 1913.

the rotor to the right until further motion is arrested by the escape of the increased pressure through a widening annular space between the plate and its facing seat. An ultimate running position is reached when the leakage through the clearance between valve and seat is sufficient to maintain a pressure at the back of the plate, equal to the set of the rotor in the opposite direction. It will be seen that the balance is quite automatic in its control, end-movement in either direction taking place until stability is reached.

FIG. 23.—*Differential Hydraulic Balancer.*

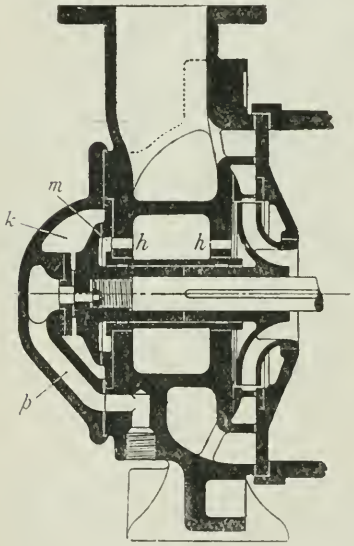
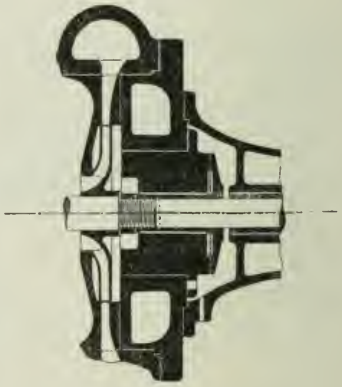


FIG. 24.—*Single-plate Balancer.* (Sulzer.)



Without going further into the detailed evolution of automatic control of the hydraulic end-balance of the turbine, it is sufficient to say that the devices adopted resolve themselves into two basic forms:—

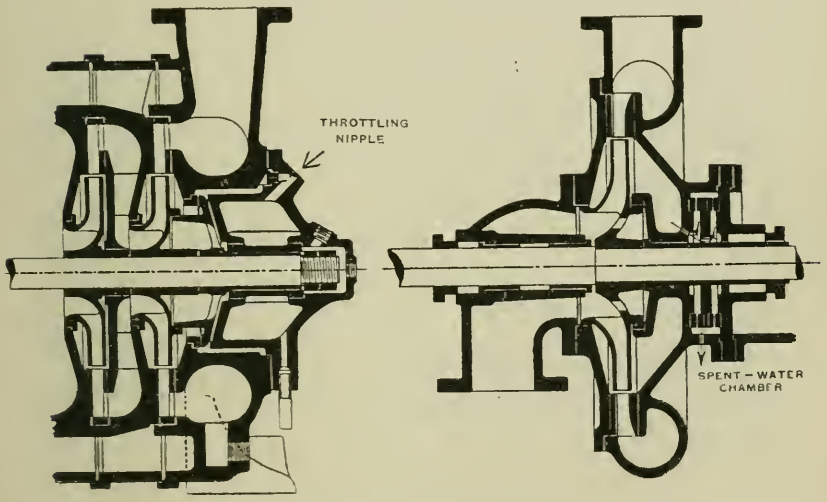
(*a*) Single acting (simple), of which the Sulzer, Fig. 24, is the general type.

(*b*) Double acting (differential), of which Fig. 23 is the type, and Fig. 13 (page 371) a modification.

Other single-plate forms are shown in Figs. 25 and 26. In the first of these the throttling agent, a small nipple, is removed to the discharge side of the plate, thus giving, in a measure, a double action to the plate; the quick action and limited travel due to the second face of the differential type is not obtained, however, and there is a possible disadvantage in the small aperture when dealing with impure water. The second example combines some of the points of both forms of balancer, but as the pressure in the spent-water chamber is always augmented by high-pressure leakage into

Single-plate Balancers.

Fig. 25 (Millington). Fig. 26 (Scheurmann) (Worthington Pump Co.).



the low-pressure side of the plate, the device is, obviously, relatively extravagant in leakage water, and must therefore be considered inefficient.

The general effect of wear on single-plate balancers is increased leakage, the outcome of which is further end-movement in one direction until finally the impeller discharges do not match their guide-vane entrances but become off-set. This tendency in hydraulic balancers led the Author to devise form "*b*," or the double-action control with which such difficulties are overcome.

Fig. 23 (also in Fig. 12, page 370) finally shows the present standard double-plate or compound differential balance arrangement. The balance-piston and regulating-valve are combined, and form a balance-disk with two active throttling surfaces floating between two fixed seatings with a minimum clearance. All the working faces are arranged vertically so as to be independent of possible vibrations of the rotary system. The pressure-water, escaping from the rim of the last impeller, passes through the holes *h* into the pressure-chamber *m*, from which it escapes along the throttling surface of the balance-valve into the regulating pressure-chamber *k*,

Continental Hydraulic Balancers.

FIG. 27.—*Differential.*

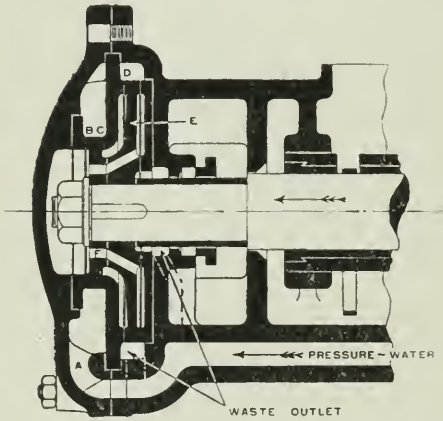
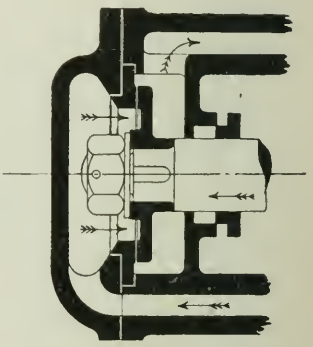


FIG. 28.—*Single-plate.*



and from there, past the small throttling surface into the escape-pipe *p*, leading preferably to an open drain, or, back into the suction-chamber of the pump. The double-plate balance appears to be the most sensitive automatic balancing device in practical use, for by it, closing up of the plate on the large throttling face causes an opening out on the small one, thus multiplying the balancing effect; a movement of a thousandth part, or so, of an inch being all that is required to counterbalance the most extreme and sudden changes of the hydraulic equilibrium of the pump. The other and most important feature of this auto-plate balance is that

it always maintains the central position of the impellers, even if the throttling surfaces become worn, as is invariably the case when pumps are working on gritty water.

A Continental form arising out of this type is shown in Fig. 27, and it is obvious that the same remarks concerning extravagance in leakage water apply to this example as to Fig. 26 (page 381). The arrangement of the balancer at the end of the spindle outside the external bearings is not a good one, for the reason that a high-pressure gland at the delivery end of the pump is still required, and an additional gland also for the balancer-housing.

A single-plate balance made by the same firm as the above is shown in Fig. 28, and this too is open to the same objection as regards disposition on the spindle.

It is usual to make hydraulic balancers with renewable faces at the points of close-running, so that the appliance can be readily repaired after wear has taken place. As regards the best material to be used for the renewable part, some considerable experience is needed before a decision can be made. Gun-metal was probably the first material to be used, but cast-iron, cast-steel, and hard bronze have all been tried, the present practice usually being to fit hard bronze. The ideal material is one which is hard and "short," and has a very low coefficient of friction when working in water; a material which "drags" is quite unsuitable, and apparently an incorrodible iron, one containing a high percentage of silicon or a nickel steel, seems to best fit the requirements. Non-metallic substances, such as red-fibre, woodite or dexine, have been considered, but any material which becomes slightly absorbent after long immersion is useless.

Sometimes difficulty is experienced in starting up large turbine-pumps on account of the large diameter balance-rings being held together in metallic contact instead of being apart in their usual running position; the result is an abnormally high starting torque for the motor and probable damage to the faces of the balancing device. When a turbine-pump with differential balancer is "stopped," the rotor will always take up a position with the balancing faces apart if the waste-water outlet, Fig. 29 (page 390), is taken to an open

drain; however, if the spent water is led back to the suction-pipe of the pump and a foot-valve is provided at the end of the suction-pipe, the whole rotor is forced towards the suction-end of the pump as soon as the foot-valve closes and the head pressure comes on. The reason for this is that the area of the balancer on the outlet side is greater than on the inlet side by an amount equal to the diameter of the spindle. For this reason, the waste water from a balancer should not be connected to the suction-pipe of a pump, or means should be taken in the pump to equalize the areas subject to the static hydraulic pressure, Fig. 12 (page 370).

IV. THE ROTOR.—As is well known, the internal design of a multi-stage turbine-pump involves a rotor comprising a number of impellers keyed on to a shaft, the shoulders or bosses of which abut on each other and are secured and maintained together by double nuts or the like at, or near, the ends of the shaft. Such portions of the shaft as are not covered by the impellers, but are subject to the action of the pumped liquid, are protected by sleeves which fit up to the impeller bosses and thus make a complete sheath outside the shaft itself, Fig. 1 *b* and Fig. 2 (*a* and *b*), etc. (pages 364–5).

This rotor revolves within the guide-chamber and housing, each impeller running at some points in the closest proximity to the partitions dividing the pressure-stages and return-guides.

These so-called running joints or neck-rings must be so maintained by design and construction as to secure in continued service the smallest possible hydraulic leakage-back from stage to stage, or mechanical loss by rubbing friction, both of which affect the efficiency and wear and tear of the pump. As a question of practical importance, the design of this combination probably follows next to that of the successful automatic hydraulic balancing of the axial end-thrust. We have in it the question of the deflection of the pump-shaft; the reinforcing effect of the surrounding impellers; the support afforded by the bushes, if any, between each stage, and the loss by bush friction (wear); the form of neck-ring to reduce leakage; and the effect of keyways and keys, etc., all independent of each other and affecting the whole.

The ideal condition is that of a rotor supported in lubricated bearings with a shaft of such sufficient stiffness between supports that the deflection, under all possible running conditions, is less than the clearance allowed at the neck-rings and intermediate bushes, so that no contact takes place between the rotating and the fixed members, this clearance at the neck-rings and intermediate bushes being kept down to the smallest possible limits. It will be seen that it is impossible to present mathematically the exact conditions with such a number of incalculable factors to take into account. For instance, it may be thought that a not inconsiderable reinforcing effect is obtained by the combination of the impellers outside the shaft as a whole. In practice, however, it is not advisable to lock up the impellers together shoulder to shoulder against each other by the end-nuts in order to get the advantage of the reinforcing effect; because, due to, amongst other things, the slight inaccuracies of manufacture in squaring the shoulders of the impellers, sleeves, etc., the result would be to throw the combination out of truth, that is, to distort the shaft. It might be contended that this need not be so, but even then provision must be made for dismantling in a mine or similar place with its obvious attendant disadvantages, and the possibility of rough treatment taking place; furthermore, as a safeguard against heating up and consequent expansion of the outside combination (bronze) against the internal (steel) shaft due to accidental contact with the intermediate bushes, running, perhaps, when the pump is empty, suitable expansion should be allowed for, and the impellers, to ensure this, must not be locked tight against each other.

The supporting effect of the bushes on the shaft, in passing through the diaphragm intermediate between the impellers, is very difficult to exactly allow for; it may be that the bushes will decrease the deflection of the shaft by a material amount, but it must not be assumed for one moment that they are bearings of such a nature as lubricated bearings. Intermediate bushes can only, in some cases, be considered as water-lubricated supports which will act as such so long as a certain low surface pressure on them is not exceeded; if too great pressure comes on, heating takes place on account of the

high speed of rotation. The effect of wear has also to be taken into account, for the hydraulic pressures at the two ends of these bushes are different and there is, therefore, always a flow taking place through them, carrying with it any sedimentary or foreign cutting matter which may be in the water. This state of things causes the supports to wear more or less rapidly so as to become almost useless, the pump eventually arriving at a condition worse than if a suitable clearance had been allowed at the first.

The Author believes that too much use is made of these intermediate supports in turbine-pump design. It is found in practice that an internal bearing to be successful must have the same water-pressure at both ends, and must be properly lubricated with good grease; it then gives excellent results. We are thus left with the fact that we are practically dependent on the shaft itself for the necessary strength and stiffness to allow of fine internal clearance, and the importance of a good design which will economically give the minimum deflection of spindle at once becomes apparent.

The factors affecting the deflection of a turbine-pump spindle are:—

Static.
- (*a*) The weight of spindle and distribution of diameter change;
- (*b*) The weight and distribution of impellers, balancer, and parts;
- (*c*) The number and span of supporting bearings; and

Dynamic.
- (*d*) In the *dynamic* condition, other incalculable forces entering into the account, such as, centrifugal forces due to the out-of-balance masses, and finally certain hydraulic disturbances.

(*a*) Should be as light as possible consistent with the necessary stiffness; in common practice spindles are practically parallel the whole length.

(*b*) Impellers and balancer should be grouped together as closely as possible, only allowing sufficient space for the water passages between the stages; also, the entire weight should be brought as close to the supporting bearings as possible.

(*c*) The span of supporting bearings plays such an extremely important part in the durability of the pump; in the possibility and preservation of fine clearances; in the whirling of the shaft and in the determination of the most economical size of spindle, that special notice will be taken of it.

(*d*) A loaded shaft supported horizontally between two bearings will "sag," and when rotated must suffer bending at every revolution. Also, there are bound to be certain out-of-balance masses in the rotor due to keys, heterogeneous composition of material, and the unavoidable variation in thickness of castings, etc. In addition, a shaft has vibrational periods due to its length and diameter, the whole question of vibration being intensely complicated by the loading and supports.

In addition to the disturbing factors mentioned above, vibrations are set up from the reaction of the impeller-vane passing the guide-vane. It is usual to reduce the intensity of this disturbance by arranging an odd number of vanes in the impeller relative to the guide-vane, and by setting successive impellers on the shaft in such a way that the point of passing-vanes is progressive throughout the series, i.e. a "lead" is given. The resultant of these arrangements is to produce a vibrating influence, which may be torsional or combined torsional and transverse, of very high frequency and enfeebled intensity and which, when properly carried out, is free from practical disadvantage. The damping effect of the water film in the neck bushes is appreciable in overcoming slight vibrations, as is conclusively shown by the different behaviour of a spindle when running a pump dry and when running it filled with water. In order for a shaft to oscillate, it must force liquid out of one side of a neck-ring, and the retardation offered by this action has a noticeable effect. For this reason, quite apart from the lubricating effect gained, it is always a wise precaution to fill a pump with water before running it, otherwise if the shaft is not a stiff one seizing-up will probably occur. The remedy for spindle vibration due to any cause whatever is always a stiff shaft.

The whole matter of whirling shafts has been examined by several investigators, prominent among whom was the late Professor Dunkerley,* but it is not proposed to discuss the matter further than to refer to the general proportions affecting the critical speeding of whirling, and with regard to this, it is well known that the length (l) of span of bearings, and the diameter (d) of the spindle have an effect varying as :—

$$\frac{d^4}{l^2}.\dagger$$

Turbine-pumps do not usually run at speeds approaching the critical speed, but the maximum safe speed for any rotor will bear a direct relation to the critical speed, in the sense that within reasonable limits it will be an equal factor for equal safety. The above relation shows then, that for a fixed critical speed a reduction in the span of the bearings results in a material reduction in the necessary shaft diameter. The effect of axial thrust—always present in a turbine-pump—in lowering the critical speed of a spindle ‡ should always be borne in mind.

With a view to considering the effect of the spindle diameter and span of bearings on the degree of fineness of the running clearances, the extent of the deflections will be considered.

The following arrangements of bearings are possible and are illustrated in Fig. 30 (page 390) :—

1. Two outside bearings.
2. One outside and one inside bearing.
3. One outside and two inside bearings.

A typical example was taken, and it was found, after drawing out the different arrangements, that the ratios of the spans of the bearings supporting the loaded shaft were, for the three cases, 1·86, 1·44, and 1 respectively, calling the shortest span unity.

As the deflection of a circular spindle is proportional to $\frac{l^3}{d^4}$, the deflections themselves for the above spans are proportional

* Trans. Royal Society, vol. 185 A, pp. 279–360, also Liverpool Engineering Society, 1894–5.

† *See* Morley's "Strength of Materials," Chap. XIV.

‡ *Ibid.*, Art. 166.

to 6·5, 3, and 1 respectively, or, the necessary sizes of spindles to maintain the same deflections are proportional to 1·6, 1·3 and 1. These results are conveniently tabulated :—

Case.	Ratio of span.	Ratio of deflections.	Ratio of spindle diameter to preserve constant deflection.
1. Two outside bearings. Fig. 30 *a* . .	1·86	6·5	1·6
2. One outside and one inside bearing. Fig. 30 *b*	1·44	3	1·3
3. Two inside bearings. Fig. 30 *c* . .	1	1	1

It will be noted that no allowance has been made for the buoyancy of the medium (water) in which the rotor revolves. The great importance of keeping the distance between bearings in a multi-stage turbine-pump as short as possible is abundantly clear from the foregoing, for, with the more or less customary type, Case 1, Fig. 30 *a*, with outside bearings at each end, it is seen that there is a deflection of six and a half times that of a pump with two internal bearings.

The direct result of this extra deflection is that the clearances between the outside of the eye of the impellers and the neck-ring must be increased to allow of the impellers revolving without touching, and this increased clearance necessarily means a correspondingly increased leakage and loss of efficiency.

Further, as has been shown, the passage of a large volume of water through the running clearance means increased wear and a tendency to push the walls apart, and therefore force the impeller and shaft still farther out of the straight with consequent friction against the adjacent neck-ring surface.

Dynamic out-of-balance may, and, very probably, will exist, even though considerable care has been taken in the workshop to overcome it; this factor is a very important consideration, and calls

for a very stiff shaft to resist its possible occurrence, because, if the shaft and impellers are thrown out due to this, then further clearance at the neck-rings is necessary or abrasion will result.

The question of the reactive effect of an impeller discharging

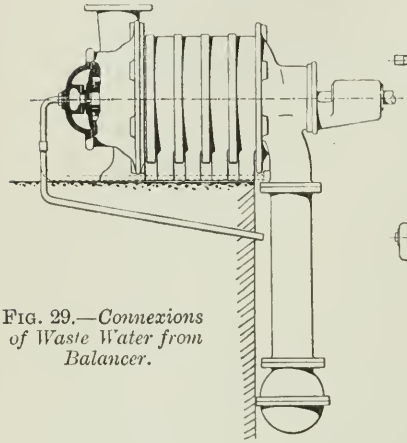


FIG. 29.—*Connexions of Waste Water from Balancer.*

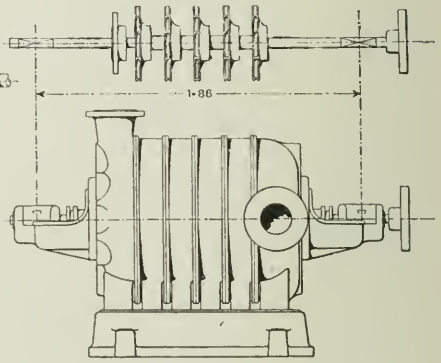


FIG. 30 *a*.

Span of Bearings—Three arrangements.

FIG. 30 *b*.

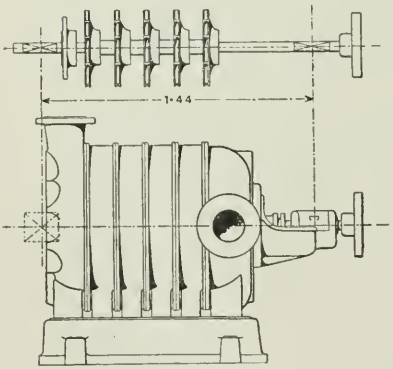
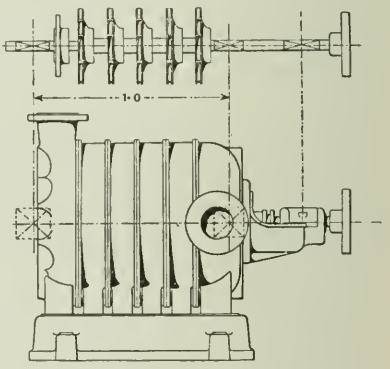


FIG. 30 *c*.



unequally round its circumference is capable of so many variations that the only practical way to overcome this also, is a mechanical construction that will resist deflection, that is, a stiff shaft.

There is a further disturbing influence which is more active in

smaller pumps, namely, the effect of the hand-packed and often ill-adjusted stuffing-box and gland; this detail is always standard for one end of the pump, and sometimes found at both suction and delivery ends. Apart from the cutting effect of such a packing on the shaft or its sleeve, it also, by reason of its compression in screwing up, tends to set the shaft out of the true centre, and so may intensify many other similar influences. The risk of outside bearings, separate or overhung, being slightly out of centre, must also not be forgotten. For very many reasons, therefore, it is extremely important that the distance between bearings be kept as short as possible, and to do this successfully, internal, specially designed, large diameter, grease-lubricated bearings are essential.

Summarizing the effect of a long distance between bearings, we have:—

(*a*) Increased deflection of rotor.
(*b*) Increased neck-ring clearance.
(*c*) Increased leakage from delivery to suction.
(*d*) Increased wear thereby, and
(*e*) Decreased efficiency.

Turning now to the dimensions of the rotor the table shows that the diameter of the shaft does not increase at a proportional rate as the length of span increases. However, a very serious increase in size (1·6 times) is entailed by the increase in span consequent upon changing from two internal bearings to two external bearings. Obviously increasing the diameter of the shaft will give the decreased deflection desired, but then it introduces a larger diameter of impeller boss and entrance eye, thus bringing about a corresponding drop in efficiency as well as more leakage owing to the larger leakage area consequent upon the increase in diameter of the boss and the neck-rings.

It is clear, therefore, that securing the stiffness of a shaft by increasing its diameter is an inefficient method, for, besides the increased area for leakage through the neck-rings, there are much worse conditions of entrance in larger and more rapidly rotating eye and inlet edge of the impeller vanes.

The desirable condition, therefore, from all points of view, is to use the smallest diameter of shaft with the shortest distance between the bearings.

Leakage between Rotor and Stator.—The leakage through the neck-rings may now be considered under different conditions and clearances. The material of these rings is the first important consideration. It is obvious that a material should be adopted in the first place of such a type that it will not tend to drag or tear if accidentally touched or rubbed by the running impeller. Material of a hard "short" nature like cast-iron will meet this condition, and in practice the smallest clearance can be successfully run with such neck-rings; they are, however, subject to corrosion, particularly with acid waters as are frequently met with in mines.

The impeller, for the reason of susceptibility to corrosion, together with considerations of the necessary strength to resist the centrifugal effects of high speed, is usually made from phosphor-bronze, we therefore frequently get in practice phosphor-bronze impellers running in phosphor-bronze neck-rings, though it is obvious that dissimilar metals would be preferable.

A phosphor-bronze impeller with a cast-iron neck-ring is a better combination, and will run with smaller clearances, but to get over the corrodibility of the cast-iron it should be specially treated, or should be similar to that indicated for renewable balancer rings.

Soft metals will not run with fine clearances, for, if any rubbing is set up temporarily or otherwise, through dynamic out-of-balance, or want of alignment from any cause whatever, the metal immediately drags and the pump seizes up. The softer the metal the greater the clearance necessary. White-metal neck-rings have been tried, but were for this reason a failure. Lignum vitæ is possible, but owing to its uncertain expansion, a liberal clearance allowance must be made; it provides, however, a material to meet special conditions (mostly stay-bearings in vertical shafts). Lignum vitæ is also frequently used in water-turbines, tail-shaft bearings for ship's propellers, etc.

The form of the neck-ring is, in practice, confined to four forms:—

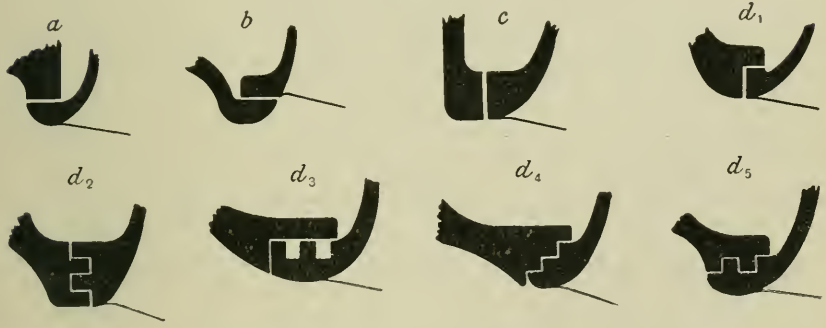
(*a*) Internal.
(*b*) External.
(*c*) Vertical.
(*d*) Labyrinth.

} Types of which are illustrated in Fig. 31.

The first is the usual form in turbine-pumps, the second is more rare, the third is practically restricted to single impeller low-lift centrifugals, while the fourth is a refined form after steam-turbine practice, and is used in some high-lift pumps.

Between (*a*) and (*b*) there is little to choose, but for manufacturing reasons (*a*) is usually preferred. Clearances of a few thousandths

FIG. 31.—*Types of Impellers.*



of an inch per side can be run with a suitable shaft, and the leakage is very slight, less than 1 per cent.

(*c*) The vertical form is used in pumps for low lifts subject to dirty water and inferior attention. It allows of downward wear of the impeller without any rubbing surface coming in contact, which (*a*) and (*b*) would not. (*d*) is an attempt to reduce leakage further by a device of the kind used on the steam-turbine.

For the reason that the labyrinth type deals with a non-expansible fluid like water, and not steam, it is far less effective than in the steam-turbine, and in fact can only restrict leakage by the formation of eddies in the channels or grooves, shock losses at

the sudden changes in area and direction, and the centrifugal effect of the rotating film of water in the narrow aperture.* Its possible advantages in this way are more than negatived by its fragile nature if it comes in contact with the impeller, such a contingency usually involving the eventual renewal of both parts. Types (*a*) and (*b*) only require a new neck-ring after wear. All types, however, are readily made with loose rubbing rings which may be easily changed.

In practice it has been found that when grooves were cut on the impeller-shoulders to secure a labyrinth effect, and at the same time the neck-rings were left plain, that after running for some time the marks of the grooves were always found on the neck-rings; the difference between the worn surface and the original surface often being as much as ten thousandths of an inch, showing in these cases a deflection of the shaft greater than the calculated value. In these particular cases the fact that the grooves appeared all round the ring to the same depth, indicated that they were due to actual deflection and not faulty alignment, the reasons for which may be found in the causes already enumerated and in mechanical inaccuracies.

When interlocking grooves are used, the difficulties of erection and dismantling are increased unless the interlocking is in the plane at right angles to the axis of the pump; any other arrangement is of course only possible with horizontally split pumps or with split or divided neck-rings.

V. THE BED, and other details. In the past, less importance has been placed in the effect of the bed-plate than is now the case, with the result that frequently very shallow depths were used. These early beds were much too weak to resist warping strains due either to the unevenness of the foundation surface, the "pull" of holding-down bolts, or to the reaction of the torque from the motor-end. It was a common practice to design bed-plates in the form of two separate horns or branches, and in this case deeper sections were generally employed. This form of design, while undoubtedly stronger against bending, depends too much upon the skill of the

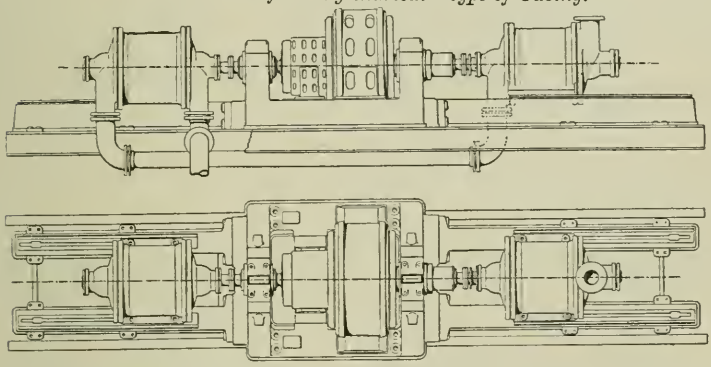
* Unwin's "Hydraulics," page 59. 1907 Edition.

plant erector and the solidity of the foundation to give always good results.

It must be obvious that if fine internal clearances, such as have been dealt with, are an advantage, then the common base for the lot—the bed-plate—must be of a nature to ensure the clearance remaining concentric and also to maintain true alignment with the operating motor. For these reasons deeper sections than are usual are an advantage.

With the "ring-type" of casing the bed-plate plays a more important part than with the "cylindrical" type, on account of the lesser intrinsic rigidity of the contiguous type and the greater necessity for a plane true surface in erecting; hence the importance

Fig. 32.—*Bed for "Cylindrical" type of Casing.*



of deeper sections becomes more pronounced still. It is now customary to have supporting feet on each "ring" of a multi-chamber pump, probably more for the convenience of re-erection in awkward situations than for any necessity for actual support. Chambers, when dismantling or re-erecting, require to be slipped along the spindle and bed, and for this reason it is usual to machine the latter for its full length.

An example of substantial base-plate provision for a "cylindrical," casing is shown in Fig. 32, and a bed as recommended for a "ring" casing pump is shown diagrammatically in Fig. 33; in cases where there is a heavy torque from the driving motor, diagonal ribs as shown are most valuable in resisting twist on the bed.

Bearings.—The almost "standard" speed of 1,450 revolutions per minute for operation of turbine-pumps does not require water-cooled bearings. For outside bearings good design oil-ring lubricated type as shown in Fig. 34 are used, a recommended feature being the

FIG. 33.—*Bedplate for "Ring" type of Pump.*

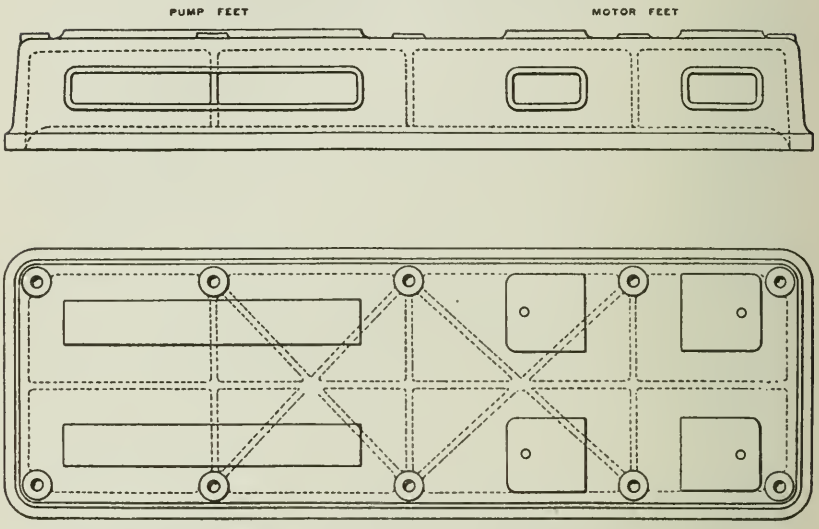
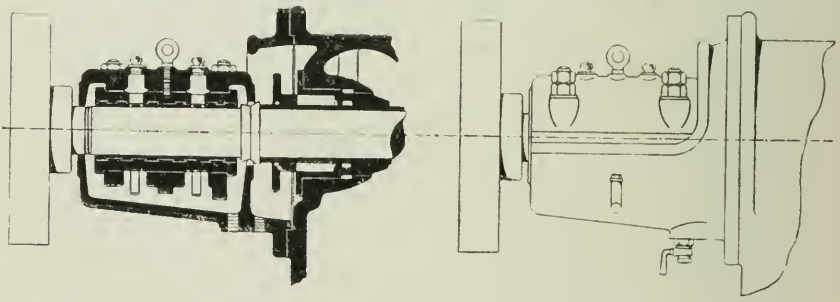


FIG. 34.—*External Bearing with Oil-ring Lubrication.*



provision of a thrower or guard to prevent water from a leaking gland entering the oil-well and washing out the oil.

As has already been stated, to keep the distance between shaft supports as short as possible involves the use of internal grease-

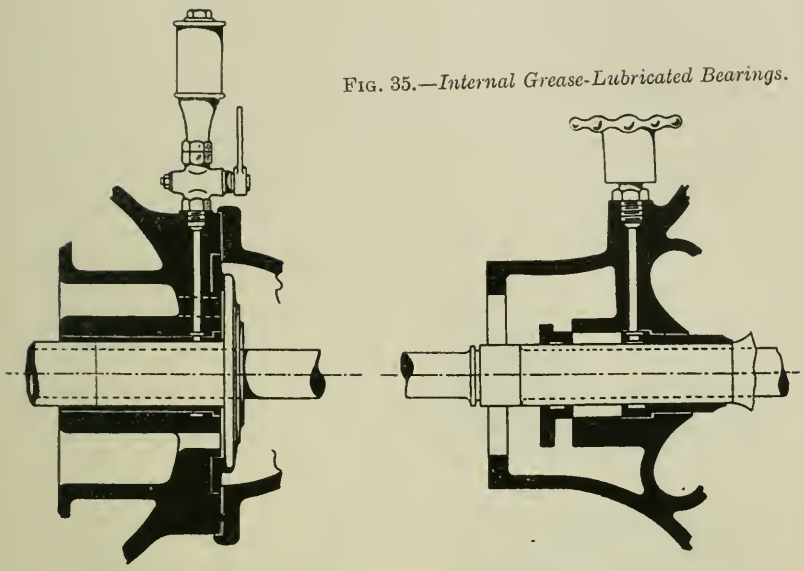
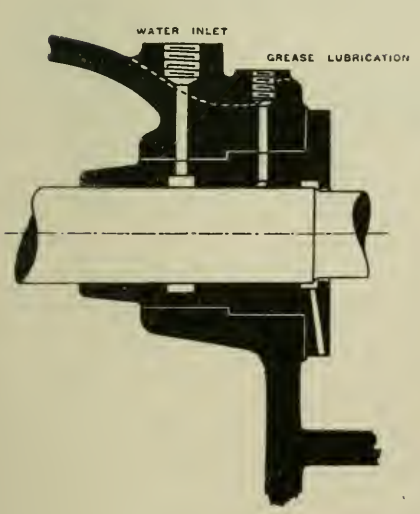


Fig. 35.—*Internal Grease-Lubricated Bearings.*



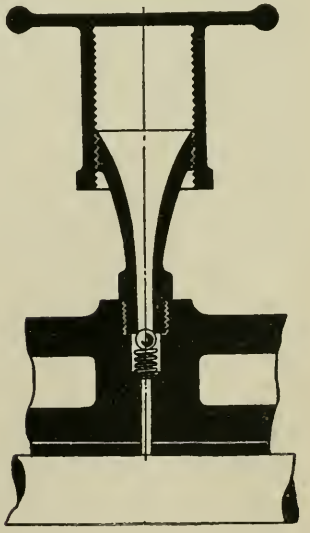
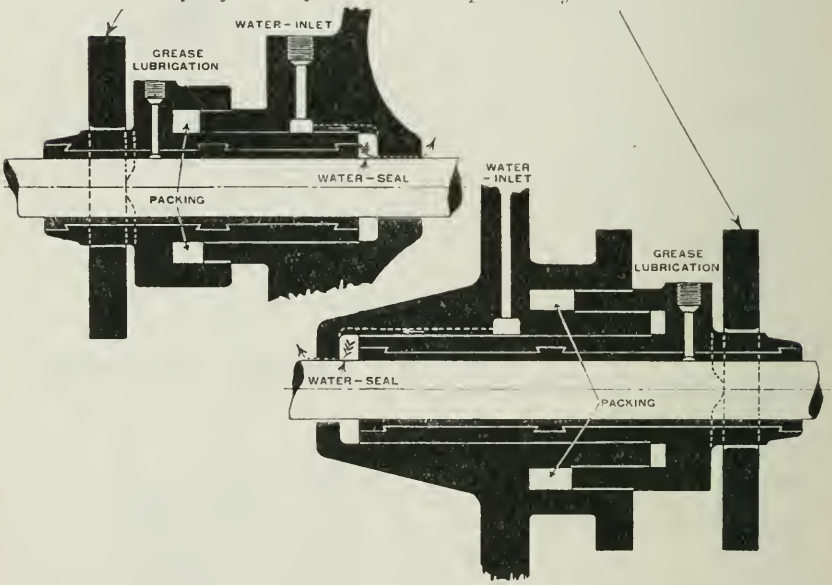


Fig. 36.—*Hand-operated Grease Lubricator.*

lubricated bearings, typical designs of which are shown in Fig. 35. This type of bearing, when carefully designed, can be made to operate very satisfactorily in practice despite a contrary view held for some time by some constructors. The important point of arranging for similar hydraulic pressures at each end of an internal grease-lubricated bearing, so as to prevent any risk of flow through the bearing surfaces, must be rigidly followed for best results.

FIG. 37.—*Suggestions for Special Gland Bearings.*

Loose flange bearing on two rounded points only.



The charging and supplying of grease to internal bearings requires proper lubricators. The ordinary "Stauffer" is unsuitable, though generally a hand-operated appliance of simple design (such as Fig. 36) meets the case; a long trumpet-shaped entrance is essential in order to force against the high pressure due to the head of water against which the pump is working. The selection of a good quality grease lubricant suitable for operating in water, plays an important part in the satisfactory lubrication of a bearing.

Stuffing-Boxes.—It is doubtful if one can accept the customary stuffing-box and gland as good and satisfactory practice. The functions of this detail are, on the suction or vacuum end, to prevent air-leakage into the pump, and, on the delivery end to prevent pressure leakage from the pump. The action of "packing" on rapidly rotating shafts is so severe as to demand the omission of a stuffing-box wherever possible. On the delivery end of a pump it is easy to avoid a gland either by closing-in the spindle, Figs. 3, 23, and 25, or by so reducing the pressure in passing through an hydraulic balancer that there is no pressure against which to pack, Figs. 4, 12, and 13.

Some constructors have done without stuffing-boxes at both ends of the pump for some time, and they generally rely on pressure water from the pump itself supplying the necessary packing to prevent access of air to the suction inlet. The possibility of difficulty in starting up a pump before the water packing is available should be carefully considered when this method is adopted.

Suggestions for the substitution of a special form of grease-lubricated gland-bearing without any packing in contact with the rotating spindle, are shown in Fig. 37. Though these bearings are not arranged with end-pressure in equilibrium, there is no tendency for water to pass over the bearing surfaces, as leakage will take place from the water sealing ring to the reduced pressure of the suction inlet.

The Author feels that the Paper has run to somewhat greater length than he at first anticipated, but he hopes that some interest may be aroused, and perhaps some useful discussion on the details of the highly important modern turbine-pump.

The Paper is illustrated by 37 Figs. in the letterpress.

Additional Note.

Mr. ALAN E. L. CHORLTON, after reading the Paper, said he had not found space in the Paper to deal with the theoretical aspect of the turbine-pump design comprised in the impeller guide-passages, etc., and he hoped that some of the members present would, in the course of the discussion, speak upon that side of the subject. The turbine-pump had attained an efficiency which was well known, but it was a good many per cent. less than that of the water-turbine. General practice proved that the turbine-pump was gradually driving out of the field all the other types of pumps. Its greatest enemy probably was prejudice, but nevertheless the turbine-pump was gradually being fitted for almost every duty of which it was possible to think. Hence, it ought to be the object of all turbine-pump manufacturers to raise the efficiency as near as possible to that of the water-turbine.

He also desired to refer to the necessity of research being carried out in connexion with turbine-pumps. At the present day a number of bodies had come into existence for the purpose of carrying out research in very many subjects. He did not know whether research fell within the purview of the Institution of Mechanical Engineers, but, if it was agreed that the turbine-pump had assumed the very important position to which he had referred, it seemed to him it ought to be the subject of special research just as much as the internal-combustion engine. If the efficiency of the turbine-pump could be raised by even 5 per cent., it would, in total effect, be of enormous value represented in pounds, shillings, and pence. He desired to say, in conclusion, that he had received considerable help in the writing of the Paper from Mr. Owen Price. Owing to the pressure of work he had experienced the greatest difficulty in finding time to write the Paper, and if it lacked anything he hoped this would be borne in mind in the criticism.

Discussion.

The PRESIDENT said it was the duty of the President to propose a vote of thanks to the Authors of all Papers read at the Institution, but on the present occasion the Members had taken it out of his hands by according their thanks spontaneously. He wished to associate himself with them.

The Paper was one of a kind he would like to see more often, as it described the results of perseverance in overcoming practical difficulties. It contained the kind of information that was hardly ever given in text-books, and yet was quite as necessary to good designing and to successful working as the matter found in text-books. The Paper, he thought, formed a very fitting supplement to Dr. Unwin's classic Paper which had been referred to by the Author. It also contained a warning to students who came fresh from college, feeling in their own minds their strength and ability to solve all sorts of problems from the knowledge they had gained there. If they relied solely on that knowledge, they would probably come to grief, not because the formulæ on which they relied were wrong, but because they could not, with the mathematical tools at present available, take account of all the conditions which arose in practice. Having said so much in favour of the Paper, he desired to act the part of a candid friend and say he regretted that, instead of the diagrams which were given in the Paper, the Author had not given drawings. Whether it was due to want of time for preparing drawings or from the desire for secrecy which undoubtedly prevailed among engineers in the present day, due to the fear of communicating to outsiders the knowledge they had themselves attained with a great deal of trouble, he did not know. It was a very natural feeling, but he did not think it was one that was of benefit to British engineering, and he hoped the time was coming when that kind of secrecy would cease to be considered necessary.

Professor E. G. COKER, D.Sc., F.R.S., after congratulating the Author on his admirable Paper, said he desired to ask two questions, the first of which related to Fig. 5 (page 368). Reference was

(Professor E. G. Coker.)

made on page 367 to Professor Gibson's conclusions in regard to the best divergent angle of guide-vanes, and Fig. 5 showed two guide-passages and the lines tangent to them. He desired to ask the Author how he selected the points to which he drew the tangents to get that angle, because he had been rather puzzled by the Fig. in that respect, and possibly other Members had been perplexed also.

The second question he wished to ask had reference to the statement made by the Author with regard to the best efficiencies which had been obtained with a turbine-pump. If it was possible, he would be glad if the Author could furnish figures of the efficiencies which had been obtained in the latest practice in turbine-pumps for a wide range of powers and heads.

Mr. T. Y. SHERWELL said he was afraid he could not add very much to the statements made in the Paper, because, although the Paper dealt with a type of pump with which he had been associated for a large number of years, it had been exhaustively dealt with by the Author. He desired, however, to support the Author's appeal that further research work should be carried out, with the object of improving the efficiency of centrifugal or turbine-pumps. Efficiencies as high as possibly 85 per cent. had been secured on representative pumps, but there was no reason why on the larger sizes with greater quantities of water the commercial efficiencies obtained should not approach 90 per cent. Manufacturers of turbine-pumps at the present time were so fully occupied in producing the pumps required for various purposes, that they could not possibly afford any time for research work. Those remarks only applied, of course, to war times, and probably would not apply to peace times, but he felt there was room for independent research work carried out on the lines of Dr. Gibson's work, to which reference was made in the Paper.

He thought a great deal too much was made of the difference between the two types of casing—the ring casing and the divided casing. In his own experience there had never been any trouble with them, although arguments were frequently heard against

either one or the other type. Personally, he thought it was not a subject for discussion at all; there was nothing to be said in favour of the one as against the other.

Mr. HUGH MUNRO said that Professor Coker had asked a question as to the efficiency aimed at in centrifugal pumps. He desired to extend that question and to ask the Author for how long the efficiency was maintained in a centrifugal pump. In reciprocating pumps he understood the efficiency was maintained almost indefinitely, but in centrifugal pumps, especially when used with acid or gritty waters, the tips of the propellers wore and the efficiency rapidly fell off. He would be glad if the Author could give some figures as to the length of life of the propellers, and the time over which the highest efficiency lasted.

Mr. PERCY GRIFFITH said he was interested in the question dealt with in the Paper to the extent that he had had some experience in designing pumps to work in bore-holes as distinct from wells. The experience thus obtained had been varied, and the results with different designs had been most interesting. While personally he had not had experience with turbine-pumps, he had seen a turbine-pump applied to a bore-hole, and he had heard from the gentleman who was using it that it was successful. The Author had dealt with the construction of the pump, and but for his subsequent invitation to discuss general points he would not have asked the question he proposed to put. He wished to ask whether the Author had any recent experience in regard to applying turbine-pumps to bore-holes, which involved their being suspended from the surface and operated by a vertical shaft. It would be interesting to know the limit of depth to which those pumps could be usefully applied. The usefulness of the pump lay in the convenience of fixing, and in the possibility of obtaining a very large capacity in a relatively small bore-hole. Those factors were very important in water-works practice, and any information the Author could give in that respect would be of much interest not only to himself but to others.

Mr. DRUITT HALPIN said there was one application of the turbine-pump which he had never seen applied in this country, but which he saw very fully described with drawings * just before the war. In that case a turbine-pump, which unfortunately was made in Germany, was working on a large station—he did not know whether it was a dock or a railway station—in connexion with a number of cranes. He presumed it was only being tried tentatively, because a moderate pressure of only about 450 lb. was used, roughly about half of what was used in such work in this country. It seemed an exceedingly simple arrangement and capable of further development.

Professor P. M. BAKER said that, as some remarks had been made in connexion with the question of the efficiency of turbine-pumps, he thought it might be of interest if he stated that he had had some tests made a year or two ago on a turbine-pump in India, of German manufacture. The manufacturers definitely stated that the efficiency was 70 to 75 per cent., but the best figure for the actual efficiency obtained on the test was about 56 to 57 per cent. He did not know whether that was a result of the German manufacture or whether it was due to the people who reversed the figures. He desired to ask the Author whether there was any reason for supposing that in acid and gritty waters, with which they were much troubled in India, difficulties would arise from electrolytic effects due to the phosphor-bronze impellers being more or less in contact with the cast-iron rings, steel shafts, and other such parts. His experience seemed to point to a general conclusion that corrosion was almost always, if not always, due to electrolytic action of some sort, a point which seemed to have an important bearing on the design of pumps or other appliances which came in contact with waters that were not chemically neutral.

The PRESIDENT said that no reference was made in the Paper to the question of couplings, which were a very important part of the

* Zeitschrift des Vereines deutscher Ingenieure, 1913, pages 1005, 1052.

centrifugal pump. It would be of great interest if any of the members could state the right kind of coupling that should be used with turbine-pumps.

Mr. Mark Robinson (Vice-President) thought it would be of interest if Mr. Griffith would state what means were used to prevent revolution of the casing of the turbine-pump which was installed in a bore-hole, to which he had referred. Did the bore-hole tube itself act as the delivery pipe, or was there a separate delivery pipe inside the bore-hole? It would also be of interest to know what the diameter of the bore-hole was.

Mr. Percy Griffith regretted that he could not answer Mr. Robinson's questions off-hand, but he would be pleased to let him see at his office a description of the plant.

Mr. Robert Thomlinson said that, as a question had been raised with regard to the pumping of water out of bore-holes, he thought it would be of interest if he stated that in the north country five or six years ago a pit was pumped out with a turbine-pump made by Mather and Platt, and it took over three years to do so. The pump was suspended on two cables, and an electric cable was run by electricity.

Mr. Mark Robinson said that in that case the hole was something very much larger than a bore-hole.

Mr. Thomlinson thought it was a bore-hole to a certain extent.

Mr. A. J. Bremner desired to say, in reply to the President's request for information in connexion with couplings, that, while he had not designed a turbine-pump, he had handled several of them fitted with different kinds of couplings varying from an ordinary solid-flange coupling to a hook-joint. In most cases it was certainly better to use a flexible coupling, but it was essential to see that the coupling was properly balanced. He had known the

(Mr. A. J. Bremner.)
outside bearings of the pumps to be ruined in a very short time, due entirely to the fact that the coupling was out of balance.

Mr. G. W. THOMPSON was not quite clear whether the Author, in his verbal explanation, stated that Figs. 27 and 28 (page 382) were Mather and Platt pumps.

Mr. CHORLTON replied they were not. Both Figs. 27 and 28 referred to pumps of German make. Mather and Platt's pump was illustrated in Fig. 23 (page 380).

Mr. THOMPSON also asked, in connexion with Fig. 19 (page 378), "Hydraulic axial balancing device" for hydraulic pressure, whether the Author recommended any particular pressure.

Mr. CHORLTON said it referred to the head water of the turbine discharging into the tail-race of the turbine.

Mr. THOMPSON said the Author had given the names of the makers of the pumps, but it would have been a great advantage if he had also given the percentages of efficiency obtained with the various pumps mentioned in the Paper, because from his experience of working with turbine-pumps there was a very great difference in that respect.

Captain H. RIALL SANKEY, C.B., R.E. (Member of Council), said that he desired to ask the Author whether any steps were taken in Works to balance the rotors against dynamic want of balance, in the same way in which the rotors of steam-turbines were balanced. There were many well-known ways of doing it, and he would like to know whether that was done in the case of the very long shafts with very heavy disks; it seemed to him that the long shafts carrying heavy disks required for turbine-pumps would require that kind of balance. It also seemed to him that the title of the Paper should not be "Notes on the Construction of Turbine-Pumps," but "Notes on the Design of Turbine-Pumps."

Mr. A. E. L. Chorlton, in reply, said he appreciated very much the kind manner in which the members had discussed the Paper; they had not dealt with it in perhaps quite the keen way which it would have been dealt with in the north of England. People were much kinder in the south. The President had asked why the sketches in the Paper were not made more in the form of working drawings or dimension drawings. He quite agreed with what the President had said in that respect; he said it was to be regretted that such drawings were not more frequently published. Manufacturers, however, were always apparently afraid of giving their experience away. In the present instance he did not think he quite fell within that category, because he was not now engaged in manufacturing turbine-pumps, so that could hardly be charged to him. With the present shortage of draughtsmen, however, it was impossible to get the drawings made; it was necessary for engineers to get along in the best way they could with the material available. To have made good drawings for his Paper would have meant beginning at the very beginning, that is, practically designing the pumps and then making drawings for them, and that was more than he could possibly have undertaken at the present time. He quite agreed with the President, however, that engineers ought to keep less back in this respect, and he inclined to the opinion that there was an increased tendency to this view at the present time. Personally, he did not think engineers lost by being more open in their disclosures.

He quite admitted, in reply to Professor Coker's criticism, that Fig. 5 (page 368) was somewhat misleading. Although the diverging angle was supposed to be tangent in both cases to the curves, it did not look from the Fig. the right way to express it; he had since reconsidered it, and found it was correct for a centre being drawn through the passage. Then Professor Coker had asked for the efficiencies of turbine-pumps. He thought it would have been more interesting if some of the representatives of makers of turbine-pumps present had replied to Professor Coker's request for this information. The variation in efficiency was dependent upon the size. At the present day a 500-gallon turbine-pump

(Mr. A. E. L. Chorlton.)

would have an efficiency of about 76 per cent., while a water-turbine under the same conditions would have an efficiency of possibly 84 per cent.—8 per cent. more—or it might be even greater. It was contended as a rule that the reason the one was better than the other was that with a water-turbine there was a convergent flow, and with a turbine-pump a divergent flow, the latter producing eddies and the other suppressing them, but he did not think that statement ought to be taken altogether for certain. The efficiencies of turbine-pumps had been greatly increased during his time, and he was convinced that by detailed investigation they would be increased still further. If any of the makers could give the latest figures of high efficiencies, he was sure it would be of interest to the members.

He wished that Mr. Sherwell had spoken at greater length, because he had a very wide experience of the subject, and as he was in the business at the present time he must know the very latest figures. Mr. Sherwell had seen turbine-pumps in use both in Canada and in this country, and could have usefully dealt with many of the points that had been omitted from the Paper.

In reply to Mr. Munro (page 403), he desired to say that the maintenance of the efficiency of a turbine-pump was a very variable factor. It was obvious that if a well-constructed pump had to deal with perfectly clear water and was reasonably attended to, it should have minimum wear; in other words, pumps dealing with waterworks water ran for years without being touched. But as soon as that condition was departed from, increased possibilities of trouble existed. If a turbine-pump was used to pump gritty water, wear would very quickly take place. Many engineers had ceased trying to pump the water that they used to pump in olden days direct through the pump, and had provided a sufficiently large reservoir or settling pond in front of the pump in order to give it a chance. But even under those conditions the results varied.

Referring to pump design, he could hardly say that the art had now arrived at such a fine state that it was possible to apportion precisely to each individual part of the design its correct value, yet we had come very near to this, and research should do the rest. In

so far as he had gone, he had tried to point out in the Paper what he thought, for one thing, should be done, that the shaft stiffness should be increased. By that means it was possible to get fine running clearances; and if that was the case, a minimum amount of water passed through, with the result that there was minimum wear, and that was the object which engineers had in view. He believed that, in many designs of pump, too much was allowed for the intermediate neck-rings to carry; if this support had been dispensed with entirely, so to speak, and the bearings had been made stiff, the difficulty would have been got over. If the designs of steam-turbines through a number of years ago were looked at, it would be found that during that time the shafts had increased in stiffness enormously. He remembered that in the early days it was not at all an unusual thing for a steam-turbine to seize up, due to want of shaft stiffness, and he was inclined to think that progress in regard to turbine-pumps ought to be made in a similar direction. Mr. Sherwell might have informed Mr. Munro how the efficiency of the pumps in the Montreal Water Works was maintained, as regards wear and tear, when pumping clean water only.

Mr. Percy Griffith had asked a question connected with the use of turbine-pumps in bore-holes. An early pump of this type which the Author had to do with was put down in a bore-hole in Moscow 18 years ago, and it ran very well. It was an 8-chamber pump, entirely suspended from the rising main within the bore-casing. It was an ordinary bore-hole lined with a tube of between 18 inches and 24 inches diameter. As it was a long time ago, the pump was a fairly big one as regards overall diameter, but a smaller size could be put in now. The shaft running down the cast-iron rising main was supported at the joints. The pump was an old Reynolds pump. Originally the trouble was not with the pump but with the ordinary bevel-gearing that was fitted at the top to transmit the drive from the steam-engine. The pump ran for years, but eventually, as water ceased to flow up the main, it was taken up. It was drawn up pipe by pipe, and it was then found that the shaft had rusted through in one place, so that the pump

(Mr. A. E. L. Chorlton.)

had stopped. When a new shaft was put in, it ran all right. Though it was possible at the present time to utilize much smaller pipes, it was still necessary to be careful in installing pumps so that a lot of sand was not disturbed in the first starting of the pump. The bore-hole must be kept clean, because if a pump was put down and suddenly started up to full output, a violent suction was created, and if a lot of sand was present, this was drawn up and the pump was worn out in a very short time.

In regard to Mr. Halpin's remarks, he desired to say that the highest pressure used in his (the Author's) day was something over 1,000 lb., the pumps running quite well at that pressure. It was then the intention to install them on battle-ships for all the high-pressure turret work, because they were self-regulating. It was possible to shut off the main and for nothing to occur in the way of undue pressure rise. Such pumps were electrically driven so that it was not necessary for steam-pipes to be taken down to the ship's bottom, as in a steam installation. The present hydraulic engines were in the ship's bottom and the steam-pipes had to go down to them. So that the pumps to which he had referred offered many advantages, and possibly they might now be used for the purpose he had indicated, although he did not know. He did not think there was any difficulty about using them for any pressure up to about 1,500 lb.

Two of the speakers had referred to the efficiency of German pumps. The pumps referred to in the Paper that were of any good were not German pumps, so that he did not think the remarks that had been made in that connexion with regard to efficiency quite applied. Years ago he came across a similar case, in which a high efficiency was claimed, while the result on test was nothing like the figure quoted. Nevertheless, there were very good makers both in Germany and Switzerland, and there was no doubt that with some of the better designs, good figures were obtained. With a pump of a thousand gallons or more, 80 per cent. efficiency ought to be approached. The maintaining of the efficiency was simply a question of keeping the water clean.

Professor Baker had asked whether electrolysis occurred through

the presence of acid water. It was very difficult to say. The life of a pump working in bad weather was in any case affected, but it was difficult to balance up the amount of deterioration due to each element present. Personally he did not know that there was electrolysis, but he was not prepared to say, without reference to his old notes, what the proportion, if any, was likely to be. When gritty water was present, it was always advisable to put a settling-tank down in front of the pump to settle the water before pumping.

Mr. Robinson had referred to the question of bore-holes. He did not remember the smallest bore-hole that turbine-pumps had been put into, but he thought the outside diameter of a 500-gallon pump was, speaking from memory, 23 inches. With a standard type of pump in a bore-hole, an output might be obtained of nearly 1,000 gallons a minute, which was very high for a bore-hole of that size. It was possible now to reduce sizes to go down to 15 inches bore.

The question of couplings was a very interesting one, and, although he had not mentioned it in the Paper, he had thought of it. In the early days of turbine-pumps, flexible couplings were used, and it was generally thought that they would successfully run out of line; but as time went on, he came to the conclusion that flexible couplings should not run out of line any more than solid couplings. Such incorrections tended to produce a side drag on the bearings, and wearing took place due to the side action of one being out of line with the other. Difficulties might also have occurred due to balance. In the type of flexible coupling then used—a sort of interlaced belt—it was in practice found impossible always to lace it up evenly, an uneven drive being obtained which had the effect of pushing one shaft out of the straight. At the present time the best coupling was one which was almost equivalent to a solid coupling, except that the connecting-bolts had hard rubber sleeves, which acted as a sort of vibration damper. It also allowed for minute out-of-line effect, but it was better that both the motor and the pump should remain in line.

Captain Sankey had referred to the question of dynamic

(Mr. A. E. L. Chorlton.)
balancing. Personally, he used to hold that view years ago, although he did not know that he held it at present; he then thought that it was absolutely necessary to have dynamic balancing, and a machine with that object in view was obtained. It was a machine with a very limited capacity, and it was not equal to taking the larger disks, although no doubt one could have been constructed. Its application, therefore, was rather limited in the first instance. But there was something else besides dynamic balancing that always used to bother him. For instance, one might say that one would balance on the dynamic machine up to speed; but what was to be done if by any chance, the wheels being cast, the discharge from the periphery was variable, more in one place than in another? Captain Sankey might say that that did not wipe out the incorrection due to the other wrong; but nevertheless the fact remained that, with dynamic balancing, a certain amount of expense was involved, and the engineer began to wonder whether it was worth while when the other condition had to be taken into account. The balancing machine was kept for a considerable time, but in the end it was found hardly worth while to continue with it. He believed the general practice in workshops was not to balance dynamically, but simply to balance statically, individually, or probably collectively all together, and not to do anything further. He did not think generally that the working speed was high enough to warrant it; it was generally below the critical speed. It was also below the worst effects of out of balance due to the impellers themselves.

In the last place, Captain Sankey had criticized the title of the Paper. It was always a difficult thing for an Author to find a title for his Paper, although sometimes he composed a much better title than Paper. In the present case he hoped those conditions were reversed. If he had used the word "Design," as Captain Sankey had suggested, there would have been no excuse for his not beginning at the beginning, and dealing with the question of how to design an impeller, why he would design it in such a way, where the variations would be, why the angle of entrance should be this or that, and so on. He considered that as being the

principal basis of the design, and as he had not dealt with it, he had decided on "construction" instead of "design." At the same time he agreed that it might have been better to have given the Paper another title.

The PRESIDENT said that if the Author and Mr. Sherwell, both of whom had referred to the advisability of research being made into the question of centrifugal pumps, had consulted the Journals of the Institution for January and May, they would have seen that a sum of £500 had been given by Mr. Williamson, a Member of Council, to be devoted to research, and that the Council had asked members to suggest researches to which the gift might be applied. If any member desired to have a research made into a particular subject, all he had to do was to send notice of the fact to the Council before the end of September, and it would receive very careful consideration.

Communications.

Mr. A. MARSHALL ATTACK wrote that in order that the radial return passages shown in Fig. 6 (page 368) could be used efficiently, the water would have to leave the diffuser in a radial direction to prevent a loss of head due to shock. The writer had never favoured this design of return-guide for multi-stage pumps, but preferred the tangential and spiral arrangement, Fig. 7, as the diffuser passages could then be kept straighter with consequently better conversion of kinetic into pressure energy, the necessary radial direction of flow being given to the water in its flow through the return passages to the eye of the succeeding impeller.

Referring to the superiority of ring or cylindrical casings, the writer had had considerable experience with both, and preferred the ring type. Not only was it superior from a manufacturing point of view, but also from the point of view of the user, a fact which anyone who had had to dismantle a colliery pump after it

(Mr. A. Marshall Attack.)

had been in use for a number of years would agree with. Even if the cylindrical casing were lined with gun-metal, the guide-ring got cemented up in the majority of colliery waters.

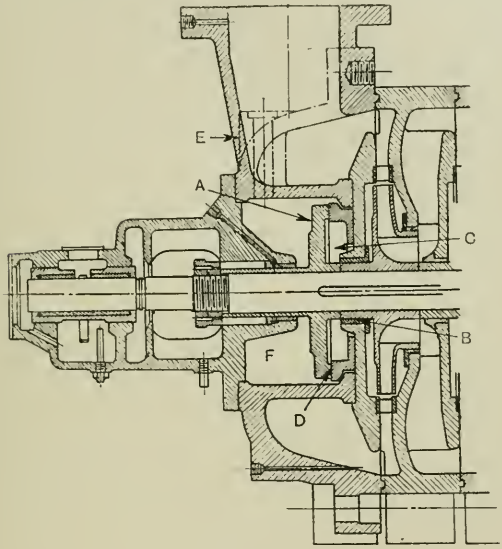
The Author stated (page 374) that "Efficiency is the prime factor in design and not apparent first cost." From this it would appear the Author considered these two factors to be dependent on one another. From the writer's experience, efficiency was quite independent of first cost or reliability. The attainment of a high efficiency called for the application of correct theoretical principles *based on experience*, whereas the attainment of reliability and low first-cost was a matter of correct mechanical design and workmanship. Some years ago the writer designed a pump which could be produced at a remarkably low cost; it nevertheless had a very high efficiency, a 4-inch pump giving 75–76 per cent. Although it was quite as reliable as a much more expensive pump, the maintenance costs would have been high, and it was not therefore deemed advisable to offer it for sale, but this was mentioned to show that first cost and efficiency did not necessarily go hand in hand.

Purchasers of pumping machinery were beginning to realize that the cheapest in first cost was not always the cheapest in the end. An example of this was seen in centrifugal coal washing pumps. The majority of continental and a good many home products were not designed for wear to be taken up, or worn parts to be cheaply and expeditiously replaced, which should be the case owing to the wear and tear on this class of pumps being heavy. In a design produced by the writer's firm, all the more vulnerable parts were easily and cheaply replaceable, and although perhaps a little higher in first cost it had replaced a great many of other makes, showing that colliery managers were beginning to realize that a pump had to be kept in order after it had been paid for.

The principle involved in all hydraulic balancers was practically the same, different makers having different methods of attaining the same end. The writer could never quite see that the differential balancer, Fig. 23 (page 380), with the internal bearing, was all that could be desired. As the running clearance between the throttling faces was only a matter of thousandths of an inch, there was

always the risk that the joint between the delivery cover and the balance-chamber cover might be too thin, causing metallic contact; alternately it might be too thick, which would cause an undue amount of leakage. Again, the internal grease lubrication required periodic attention, besides being wasteful in lubricant. Lack of attention would probably cause undue wear, with the consequence

Fig. 38.—*Self-adjusting Balancer.*



that the spindle would get out of alignment, and allow the balance disk to rub on one side of the face.

Fig. 38 illustrated a balancing arrangement, which possessed all the advantages but none of the disadvantages of the differential type. Its action was as follows: The area of the balance-disk A was such that the water under delivery pressure which escaped along the running clearance between the impeller-boss and the bush B maintained a pressure in the space C just sufficient to balance the thrust of the impellers towards the suction. The leakage-water escaped past the disk-face D, the disk running

(Mr. Marshall A. Attack.)

clear of its face under all hydraulic conditions. The impellers were consequently held in the normal running position. The balance-water was conveyed back to the suction by the balance-pipe which was connected to boss E. The pressure existing in space F was only suction pressure, hence it was possible to have a stuffing-box and the valuable addition of an automatically ring-oiled bearing at *both* ends of the pump. It did not require any fine adjustments to give the necessary clearance on the balance-disk, but automatically took up the correct position and was as near "fool proof" as any design could be. The danger of the balance-disk being held hard against its face, as mentioned on page 383, as applying to the differential type, was also absent, as the shaft was continued through the stuffing-box, consequently there was no unbalanced area exposed to cause this. The amount of water required for this type of balance was not more than 1–1½ per cent. of the total quantity pumped. It would appear that the differential type would require to be set up by skilled labour to get this result.

The writer was in agreement with the Author that the spindle of a pump should be of sufficient stiffness between supports that the maximum deflection under all running conditions was less than the clearance between the neck-rings and intermediate bushes, so that no metallic contact could take place. It had always been the writer's practice to design shafts on the basis of being supported in the two lubricated bearings only, without relying upon the intermediate bushes for support.

The hydraulic disturbance caused by the reaction of the impeller-vanes passing the guide-vanes could not only be reduced by arranging an odd number of vanes in the impeller relative to the guide-vanes, but also by allowing a fair radial clearance between the periphery of the impeller-vanes and the tips of the guide-vanes, as this allowed the absolute velocity and direction of the water leaving the impeller at a slightly varying angle to accommodate itself to the path of the guide-channels. It should be noted that only the middle water thread left the impeller at approximately the correct direction corresponding to the path of the guide-channel.

Mr. ARTHUR JOHNSON wrote that the method adopted, Fig. 12 (page 370), was a very good one for producing the stator frame, and dispensed with a fair amount of foundry labour. In fact, for small and medium size machines it was just the kind of work which could be done by machines with female operators. When describing the method adopted for rendering the throat-passage smooth so as to ensure the maximum of efficiency, the Author urged that a good surface was essential for the best results, and would always justify the increased cost (page 376). For the last few years electrical engineers had been interested in a class of compounds called Resoles and Resites.* These compounds were formed primarily, under well-defined conditions, by the action of carbolic acid and formaldehyde on one another. In view of the statement just mentioned, it would seem that this material when obtained in alcoholic solution and painted or sprayed uniformly on the guide-passages, casing, and impellers, would provide, when hardened, a nice smooth surface on those parts which were subjected to the action of the high velocity fluid. The varnish would be hardened by heat-treatment for a few hours at a temperature of about 250° to 300° F. (say 120° to 150° C.) after which, if properly carried out, the various parts should be impervious to the action of water, acids, and most alkalis. (Strong caustic soda attacked this varnish slightly.) Some appreciable saving might be made by eliminating the bronze spacing collars on the shaft and treating the rotor, as a whole, in the manner just described.

He would like the Author to state in general terms the law holding between the load and speed: and the axial thrust which would come on to the motor bearings if the hydraulic balancer failed.

The method adopted in designing the rotor and shaft of the pump was not unlike that used for the rotor of the motor driving it, especially if that motor happened to be of the induction type, with its small mechanical clearance between rotor and stator. In both cases the clearance was cut down to the lowest possible limit

* Lebach, Ph.D., Journal Chem. Soc. Vol. 32, No. 11.

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(in the case of the induction motor it was of the order of 1 to $1\frac{1}{2}$ millimetres, or 40 to 60 thousandths of an inch. Assuming that critical speed considerations could be neglected, it would be interesting to know the permissible deflection allowed for pumps, expressed, say, as a percentage of the gap or radial clearance between stator and rotor.

The Author stated (page 387, paragraph *d*) that there were bound to be certain out-of-balance masses, etc., but on looking at the drawings in the Paper, one was struck by the apparent absence of any special precautions for correcting, both statically and dynamically, as far as possible, the effect of these unbalanced masses.

Critical speed was really the main factor which determined the diameter of a shaft for high-speed machines. In present-day practice one designed a shaft such that the first value of the critical speed should be 20 to 30 per cent. above the running speed, since great damage might accrue to both rotor and stator were the critical speed to be lower than the running speed. Perhaps the Author would state in his reply whether he had a short cut to the calculations of the critical speed for any particular machine. The combined graphical and analytical method was somewhat lengthy, although the result was likely to be more correct than that obtained by the use of empirical formulæ, especially when dealing with a shaft of varying cross-sections such as was illustrated in the recent Paper by Sir Wm. Beardmore.* In addition to the reduction in the value of critical speed due to axial thrust, there was the effect of the motor rotor to be considered. As flexible couplings were mostly used as a connecting medium, they acted to some extent as a damper. In small sets which were rigidly connected, and those which ran at greater speeds than 1,500 revolutions per minute, this damping effect would not be appreciable. Could Dunkerley's method of compounding be adopted to estimate this effect? †

* Proc. I.Mech.E. 1917, page 222.

† Phil. Trans. Vol. 185A.

Of the various devices illustrated on page 393 for reducing the water leakage, type d_3 was rather favoured on account of the cost of machining compared with the other "d" types. Such a design worked very well with motor bearings and reduced the leakage of the lubricants. The machining costs could be cut down on the bed shown on Fig. 33 (page 396), by planing

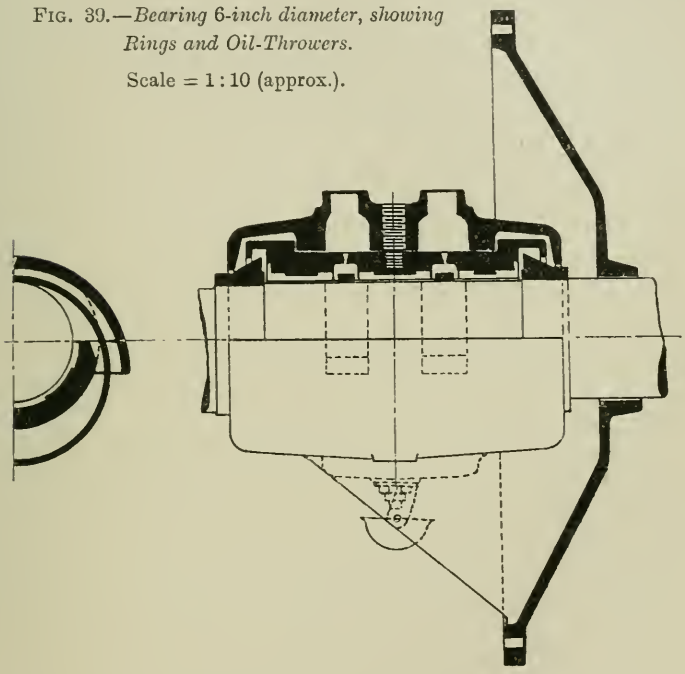


Fig. 39.—*Bearing 6-inch diameter, showing Rings and Oil-Throwers.*
Scale = 1 : 10 (approx.).

the bosses for the foundation bolts level with the facings provided for the pump and motor feet, instead of shell-cuttering each boss to a lower level.

Manufacturers of high-speed machines were giving much attention to the design of ring-lubricated bearings and in particular to those intended for electrical machines. Trouble, not confined to any one manufacturer, had been experienced due to the leakage of oil at the joints caused by the oil being flung

(Mr. Arthur Johnson.)

from the shaft-collars at high velocity on to the inner walls of the housing. Further, in some cases, for example an electrical machine arranged for pipe ventilation, there was a great tendency for the oil to be sucked into the inside of the machine, with the result that the bearing seized up.

The design shown herewith, Fig. 39 (page 419), had much to recommend its use for turbine-pump work. It would be noticed that small clearances were provided over the oil-flinger hubs and a bell was provided at each end of the bush which shrouded the flinger. The oil thrown off at high velocity was caught by this end-cover. It did not matter if oil leaked out through the bush-joint, since its velocity being gone, it could run down to the sump again. The oil-rings were shrouded by a bridge which extended below the horizontal centre line and only small inspection holes were provided in the top of the bush, so that upward splash leakage was reduced. In the outer housing, channels were provided to eliminate the oil-creepage. The provision of large oil-throwers at each end of the bush was advisable. It was not quite sufficient to have a few grooves cut in the shaft about 2 mm. deep, and 3 to 5 mm. wide, when running at speeds, such as obtained in turbo work. The use of felt-wipers should be discouraged, since they became hardened when silted up with dirt, and then charred, which eventually produced scoring of the shaft. Another feature of this bearing, and which also was present in the Author's design, was the pocket provided between the stator casing and the end-bell of the bearing housing. This allowed free access of air round the shaft and relieved the suction which, if this space were not provided, would ultimately drain the bearing.

In conclusion, he would like to ask the Author what pressure per unit area due to mass load was allowed in the grease-lubricated bearing illustrated on page 397.

Mr. G. P. MAIR wrote that it would be noticed that the Author dealt at considerable length with the question of balancing disks against end-thrust, and went into some detail regarding automatic hydraulic balance. The automatic hydraulic balance was an

extremely pretty arrangement from the design point of view, but in practice there were considerable difficulties still to be overcome. One chief difficulty was that, in practically every case, a centrifugal pump was dealing with water which was charged with fine sand, or some form of abrasive material. Owing to the fact that it was advisable to keep the quantity of leakage water passing the hydraulic balancing piston a minimum in order to obtain a high efficiency of the pump, it became necessary for the leakage surfaces to be as close together as possible. This was also accentuated by the fact that the circular leakage surfaces must be of large diameter, so as to obtain sufficient area of what might be termed the balancing piston. The result of this was that any particles of dirt, grit, or sand, quickly cut away the leakage surfaces, and although provision could be made to renew these, it was generally a troublesome matter to do so—in fact, the writer had seen a large colliery sinking pump where it became necessary to put in new balancing disks every three days.

ouble inlet-disks were complicated to make, and entailed tortuous cored passages in the outer casing, but no doubt they would make an ideal arrangement. There was, however, a compromise, and that was to build the pump with single inlet-disks balanced against each other, that is, the total number of disks were so arranged that half pointed one way and the remaining half the opposite way. This arrangement did not necessitate complicated connecting passages between the various sections, but it required a small mechanical thrust-bearing to take up any residual end-thrust. The water entered one end of the pump, traversed half the disks, and was then conducted by a pipe or passage to the opposite end, passed through the second set of disks, and finally emerged from the middle of the casing. One gland worked under the suction pressure and the other under a pressure due to only half the head, instead of the full head, as was the usual practice. A number of pumps had been built on this principle, giving excellent results, and had overcome the difficulty of serious wear with any form of hydraulic balancing system.

The Author dealt with one extremely interesting point, and

(Mr. G. P. Mair.)

that was on the standardization of designs. No doubt all makers of pumps had tried their best to come to some form of compromise so as to standardize the parts, but as the Author rightly pointed out, it was impossible to obtain high efficiency unless the disks and guide-vanes were specially designed for the duty, and this applied in particular to turbine-pumps.

The old question was raised (page 375) as to whether the water, when it entered the eye of the pump, flowed with a spiral motion or not. A short time ago the writer had an opportunity of carefully examining a double-inlet circulating pump which had been in use for some considerable time in a boat engaged chiefly in the tropics. The pump casing was split horizontally, and when dismantled was found to be covered badly with incrustation—in places to a depth of $\frac{1}{4}$ inch. The careful examination showed the exact path of the water up the suction passages of the pump and right into the eye. All eddies could be traced, and, in fact, the outside stream lines (which one read about in text books) were actually traced in the incrustation. There were a number of extremely interesting points, but there was direct proof that the water entered the eye of the disk with a swirl. The amount of whirling could be traced up the suction passage, and in the plane of the eye the stream-lines of the outside column of water made an angle of approximately 25° with the axis. Doubtless nearer the shaft the amount of whirl would be still greater.

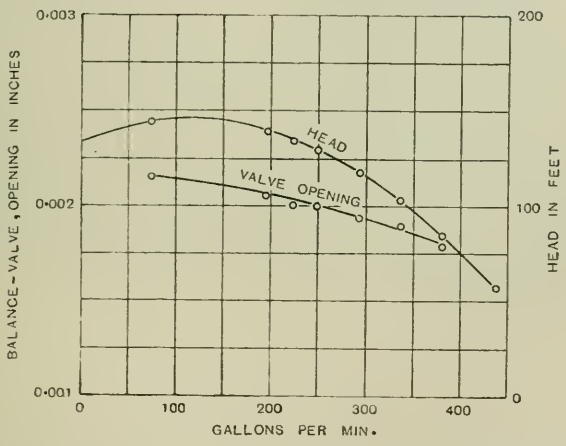
Mr. W. E. W. MILLINGTON wrote that the Paper was most interesting, although it appeared to centre somewhat round the one particular design of pump in which the Author was interested. It would have been more fair to other makes of pumps if the Author had described Fig. 12 (page 370) as showing one form of modern pump, as there were other makes equally as good and probably more modern.

The Author rightly dealt at length with the all-important balancing of the end-thrust. The Sulzer back-to-back impeller arrangement, and the arrangements shown in Figs. 20 and 21 (pages 378–9), were all non-automatic, and it was necessary to

provide some sort of thrust-bearing to carry the out-of-balance end-thrust, which was bound to appear when the head on the pump varied, or as soon as any wear took place in the pumps. The arrangement shown in Fig. 21 was particularly bad on account of the large diameter of the balance piston attached to the last impeller and the large amount of wear which took place in consequence. For this reason it was very doubtful whether the automatic arrangement shown in Fig. 22 would have been satisfactory, even had it ever been made.

It was not clear why the Author should resolve the various automatic balancing devices into the "two basic forms," as even in

Fig. 40.—*Observations made with Balancer*, Fig. 25 (page 381).

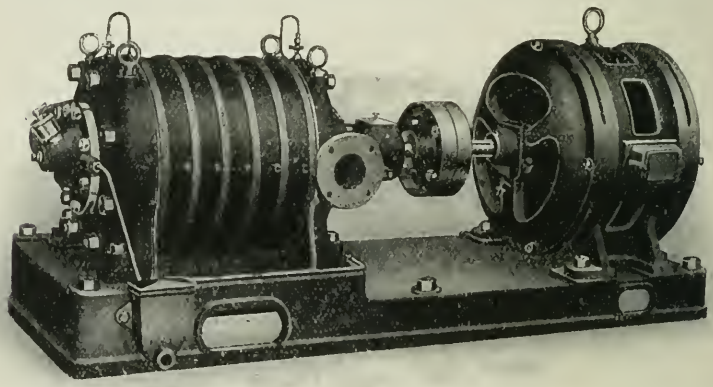


the so-called differential arrangement, in order to obtain a balance under varying heads, a definite end movement of the shaft was necessary. Possibly the amount of this movement was less in the Fig. 23 (page 380) than in the Fig. 25 arrangement; but since, as would be seen from the diagram, Fig. 40, constructed from figures actually obtained from a pump fitted with the Fig. 25 arrangement, the balance-valve opening varied considerably less than 0·001 of an inch over the whole working range of the pump, the double valve arrangement of Fig. 23 would appear to be an unnecessary refinement

in quick action and limited travel. This seemed to be borne out in practice, as the devices shown in Figs. 24 and 25 had not shown any want of sensitiveness, and as neither of them had two valve-faces which had to run with and maintain very fine clearances, constructionally they had decided advantages over the double-face type of balancer.

The Author claimed that the double-valve balancing arrangement always maintained the central position of the impellers, even if wear took place on the valve-faces. This would, of course, only be

FIG. 41.—*Pump, with waste water taken into open drain-box on bed-plate.*



so if the two faces wore at a definite proportionate rate, but even in such a case the rapid increase in the amount of balance-water required with this double-face arrangement the moment wear took place, would probably lower the overall efficiency of the pump more than the slight endwise displacement of the impellers, which took place in pumps fitted with the balancing arrangements of Figs. 24 and 25, as these balance-valves automatically followed up any wear on the valve-faces, and thus maintained a constant quantity of balance-water required.

The Author's remarks that, when a pump fitted with differential balancer was "stopped," the rotor would always take up a position

with the balancing faces apart, if the waste-water outlet was taken to an open drain, applied equally well to the balancing arrangement shown in Fig. 25. For this reason, in pumps fitted with this arrangement of balancing, the waste water was taken into an open drain-box, usually fitted on the bedplate, as shown in the illustration, Fig. 41.

The Author's opinion that the impellers should not be locked up tight against each other on the shaft, was not in accordance with the practice of others. It was found that with good workmanship there were no inaccuracies of manufacture in the impellers sufficient to cause any distortion of the shaft, if the impellers were locked up together. As a matter of fact, considering the section shown in Fig. 12 (page 370), it would be noticed that when running, the out-of-balance end-thrust on each impeller acted in a direction towards the left. All the impellers were therefore pushing against each other and eventually against the sleeve on the left-hand end of the shaft. This thrust was balanced by the load on the balance-disk acting in a direction towards the right and transferred through the sleeve on the right-hand end of the shaft to the shaft-nuts. It would be seen, therefore, that even if the impellers were not locked up by the shaft-nuts, they were locked up by hydraulic pressure when the pump was running, and any clearance left by not tightening up these shaft-nuts would all be accumulated between the right-hand end of the last impeller-boss and the left-hand end of the sleeve attached to the balance-disk. This allowed an opening for the water with which the pump was dealing, to get on and along the shaft and set up corrosion. It was preferable to lock up the whole of the rotating parts and thus receive some reinforcement as regards stiffness of the shaft.

As stated by the Author, the most desirable conditions in the design of a multiple-stage pump-shaft were that the bearings should be as near together as possible, and that the shaft must be stiff enough to stand all the dynamic forces tending to deflect it. If these were provided for, the shaft would meet all the static factors affecting the deflection. In centrifugal pump manufacture, the

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great aim was to obtain reliability, even if necessary at the expense of efficiency as regards power consumption. To obtain this reliability, a good design was of course necessary, and it was essential that the parts should be made of the most suitable metals. Hardly sufficient notice had been taken in the Paper of this latter point. The Author stated that the conditions under which pumps worked were such that we frequently got in practice phosphor-bronze impellers running in phosphor-bronze neck-bushes, but pointed out that this was not a good combination from a running point of view. The writer's firm was obtaining excellent results, both as regards running and non-corrodibility, from bronze impellers running in bronze neck-bushes. This was obtained by constructing the impellers of a bronze having a breaking stress of 35 tons per square inch, with an elongation of 20 per cent., and the neck-bushes of a bronze having a breaking stress of 37 tons per square inch, with an elongation of 5 per cent. Too much care and trouble could not be taken in testing and controlling the metal mixtures for all the parts of a centrifugal pump.

The writer's experience was quite in agreement with the Author's as far as internal bearings were concerned, and there was no doubt that if they were properly designed they could be made perfectly satisfactory.

Mr. C. S. YOUATT wrote that the Paper presented the main essentials of design of the so-called turbine-pump, and it was therefore only possible to attempt to add to the comments made regarding the relative value of certain features introduced to overcome some of the earlier difficulties, and which had now brought this class of pump up to its present high standard of reliability, as it was reliability which was lacking and sought in the earlier days of the pump rather than efficiency. Following up this point, it might be mentioned that, as it was usual with most pumping installations to provide a margin in power in the driving unit over and above that actually required at the specified duty, and again that to give this power, in the case of a motor, the motor manufacturer, in order to put forward a standard machine might

have to take a size which had still more margin, it was obvious that the motor would not be working at its highest efficiency, and therefore it was hardly necessary to consider fine points affecting efficiency, especially at the possible expense of reliability, as a slightly higher percentage efficiency might not necessarily affect the size of motor required.

The importance of balancing of end-thrust rightly received considerable attention from the Author. The early development of the multi-stage pump was slow; although Professor Osborne Reynolds invented the series type pump with guide-vanes in 1875, it was not until about 1900 that high-lift centrifugal pumps began to be installed at all freely in this country, and, with almost all pumps dealing with heads of about 200 feet and over, trouble was experienced owing to the want of proper balancing against hydraulic end-thrust. Most of the early pumps made were, like the original Osborne Reynolds pump, without any provision for taking end-thrust excepting a thrust-bearing usually quite inadequate for dealing with the thrust combined with high speed of rotation.

Methods of balancing, such as those shown by Figs. 21 and 22 (page 379), while improving matters considerably, still proved hopeless in many cases, as excessive wear on the balance piston allowed more water to pass than could be taken by the balance-valve and pipe, and although adopted in various forms, they generally gave much trouble. Towards the end of 1906 the high lift form of centrifugal pump seemed likely to fall into disrepute in collieries largely, where the pump had been extensively installed on account of small space occupied. The introduction of the system of hydraulic balance shown in Fig. 24 (page 380), however, seemed to prove the turning point, and balancing devices embodying the main feature, namely, the disk, of this type all apparently proved successful, and users of these pumps became assured again of reliability in what was the weakest point.

Regarding the Author's comments on the system of balancing shown in Fig 25 (page 381), the writer had some considerable experience with this a few years ago, and found it to be a complete success; there was never in his knowledge any trouble

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with the orifice becoming choked up, and there was the advantage that the water leaking away to waste did not increase. It would be noticed in the illustrations of this form of balancing, Fig. 25 and Fig 24, that only a small pipe connexion to waste was allowed for, whereas in that shown in Figs. 23 and 12 a very large outlet was provided, suggesting that a large quantity of waste balance-water was allowed for. It would be seen that the so-called balance-disk, Fig. 25, was in reality a valve, and that the back of the impeller took the load necessary to balance the end-thrust.

Regarding end movement of the whole rotor, causing the impellers and guide-passages to come out of line consequent upon wear of the balance-valve and face, there would not appear to the writer to be much in favour of either of the forms shown by Figs. 23, 24 or 25, as, in the first-named it did not appear likely, with water containing grit passing the faces, that the two different diameters of valve-face would wear evenly; but in any case, if balance-valves and seatings were made of proper materials and ran truly with one another, the end movement would be found to be immaterial. In this connexion it would appear that the balance-valve of Fig. 25 had an advantage, in that it had a ring integral with it which fitted into the back of the last impeller, which should conduce to the true running of the valve.

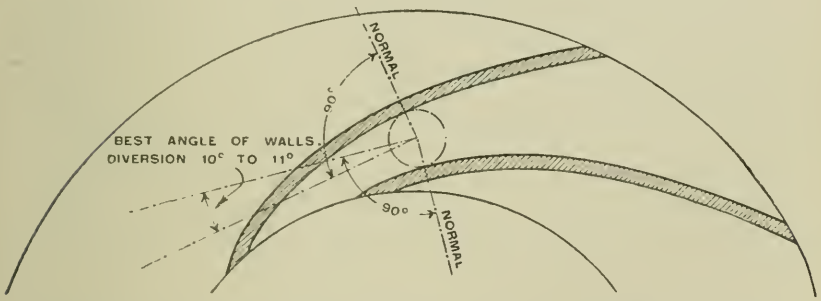
The writer would ask what total clearance was allowed in between the valves and faces in the differential form shown in Fig. 23; his experience was that with a clearance of about 0·001 inch or less at one face there was a great tendency to seize. In the illustration, the diameters of the large and small valve-faces were approximately as $3\frac{1}{2}$ to 1, so that if the openings of the large valve from its face was 0·001 inch, presumably the space between the small valve and face must be 0·0035 inch to pass the same quantity of water, assuming the pressures in each chamber to be the same; as, however, there must be a lower pressure in the intermediate chamber, it would appear that the opening of the small valve must be greater than the figure mentioned. Further, when the pump was started up, there might be a tendency to "hunt," so that the small face would perhaps wear unduly in

comparison with the "single" balance-valve — this again might account for the large balance-pipe provided.

The writer agreed fully with the remarks regarding stiffness of shaft and desirability of shortening the space between bearings. This could be accomplished by the adoption of an internal bearing of such a type as was shown by Fig. 25, and it should prove equal to all demands, as the end was sealed by the grease and the bearing was water-cooled.

Mr. A. E. L. CHORLTON wrote, with regard to Fig. 5 (page 368), upon which a comment was made by Professor Coker (page 401), that the following explanation and accompanying sketch, Fig. 42, illustrated how the divergence was measured. The expansion at

FIG. 42.—*Design of Guide-Passage.*



any point was tested by drawing a circle in the passage, touching the two walls. Normals to the points of contact were then drawn, and tangents to the walls were drawn at right angles to the normals and extended until they met. The angle between the tangents then represented the divergence of the guide-vane.

The Author noted Mr. Attack's views with regard to the inter-relation between efficiency, first cost, and reliability, but could not agree with them, stated baldly as they were. His comment would have had much more point if he had actually given particulars, or sketch, of the identical pump upon which he had achieved the result stated. The Author did not follow, from the description, the particular novelty in the type of balance illustrated by

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Mr. Attack, for it would appear that it was merely a slight modification of the well-known Sulzer type, illustrated in the Paper, Fig. 24 (page 380). From the remarks following, it would almost appear that he had not quite grasped the action of the differential type balancer, which would also indicate that he had had no great experience in working under the very difficult situations which had practically forced an invention of this type. His further point about increasing the radial clearance between the impeller and the guide-vanes was, of course, well known, but obviously could only be adopted to a very limited extent.

Mr. Arthur Johnson's remarks upon special formaldehyde compound were interesting. So far, the Author had not found any form of metallic paint which would withstand the action of the water when travelling at the higher speeds.

With regard to balancing, the Author did not think that any general law held good; each design must be investigated separately, but information which would be valuable to Mr. Johnson in this work could be obtained from the Papers referred to by the Author. The remarks anent critical speeds were of interest, but the Author did not think they called for any further comments other than the reference he had made in the Paper. There was little doubt that in this, as in other cases, individual designs must be taken into account, and practically demonstrated afterwards. The design of bearing suggested by Mr. Johnson was but a small variation of the one in the Paper, and was, the Author would take it, general electrical practice.

The Author did not agree with the point of view brought forward by Mr. Mair (page 421). He had been concerned with the manufacture of pumps of the type mentioned, namely, back-to-back, but did not find that it presented any advantages. The differential form of balance-pump was, in the first instance, brought out to meet difficulties experienced in pumping water containing a percentage of grit. It could not, however, be too strongly pointed out that every possible step should be taken to prevent the passage of grit through the pump, for in whatever type of pump was used, considerable wear in one way or another was bound to take place.

For special mine-sinking, where the difficulty of grit could be got over, the back-to-back, or double-flow type of pump, was sometimes used.

The arrangement advocated by Mr. Mair was shown in the Paper, Fig. No. 2 *b* (page 365), from which it could readily be seen that if high pressures were worked to, very considerable end-thrust might result. Mr. Mair's remarks on swirl inlet into a marine circulating pump were interesting; it would be of still greater value if he would state how much of this swirl was due to the bend of the suction-pipe, and how much to the impeller. It would also have been advantageous if Mr. Mair had mentioned the inlet angle of the vanes in the impeller of this pump, with his comments upon how the swirl affected it.

Mr. Millington's remarks on balancing (page 422) were of considerable importance, though, having particular reference to his own method of balancing, they still did not refute the point stated by the Author, that the differential balance was quicker in action and shorter in travel, whatever the amounts actually measured might be.

Mr. Millington's method, with regard to the employment of different proportions of materials of the same base for the impellers and neck-bushes, seemed distinctly a desirable way of meeting the difficulty. It was also interesting to have his confirmation of the satisfactory results he had obtained with internal bearings. It was to be noted, as against this, how Mr. Attack, in his remarks, spoke of the advantages of separate ring oil-bearings, though the disadvantages they involved had been clearly pointed out in the Paper.

The Author did not quite follow Mr. Youatt's remarks on efficiency, with relation to the motor employed. Examples of this nature could always be adduced against any point raised, but they were hardly justifiable. If the best results were to be obtained, it was always essential to consider all the points affecting efficiency. The present type of pump should provide a high efficiency along with reliability, and not make necessary a large reserve of power, which used to be considered the right thing, in the driving motor.

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The Author was glad to learn that the form of balancer mentioned by Mr. Youatt was giving satisfaction in practice. His experience with the clearances in balancer faces was noted. Here, again, the essential point was to use different materials when close running was to be obtained. It was to be regretted that more remarks were not made upon the importance of the stiffness of shafts and the adoption of internal bearings to provide this.

As regards leakage from balancers, did not Mr. Youatt attach undue importance to the size of drain-pipe? He further neglected the difference in scale of the various drawings. The leakage from any balancer would get through the ordinary drain-pipe as the back-pressure would always rise to suit. It was always a mistake to employ anything but substantial fittings in practical work, having in view the usage in mines. The gap between the faces of a hydraulic balancer was not determined by the amount of leakage water, but by the necessary pressure conditions throughout the device.

MEMOIRS.

James Meredith Austin was born in 1865. He served an apprenticeship of five years, from 1882 to 1887, with the firm of Messrs. Harland and Wolff, Belfast, and on its termination he obtained an appointment in the Indian Marine Dockyard, Bombay. Three years later he returned to England and became a draughtsman in the works of Messrs. Willans and Robinson, of Thames Ditton, subsequently being engaged as assistant engineer to Mr. Hermann Kühne, of London. In 1894 he went to Messrs. Allen, Son and Co., Bedford, and left them two years later when he took up an appointment at the works of Messrs. R. E. Crompton and Co., Chelmsford. In 1898 he was engaged by Messrs. Siemens Brothers with whom he stayed some years, and in 1907 he went to Egypt as engineer on the staff of the British Engineering Co., of Alexandria, subsequently becoming general manager of the Company in Cairo. This post he held for some years until his death, which took place suddenly at Alexandria on 23rd July 1916, at the age of fifty-one. He was elected a Member of this Institution in 1892.

Sir Benjamin Chapman Browne, D.C.L., was born at Uley, Gloucestershire, on 26th August 1839. He was educated at Westminster School and King's College, London. In 1856 he began his apprenticeship at the Elswick Works, Newcastle-on-Tyne, and, after serving his time, he obtained a situation as draughtsman under Mr. James Abernethy, being engaged on the construction of the Falmouth Docks. His next appointment, between 1863–5, was on the Tyne Pier works, as an assistant engineer under Mr. Messent, while for four and half years, from 1865–9, he was on Sir John

Coode's staff as resident engineer on the Port Erin works and on the landing pier at Douglas, Isle of Man. In 1870 he returned to mechanical engineering, and was one of several who purchased the works of Messrs. R. and W. Hawthorn, on the Tyne. The firm soon acquired St. Peter's works, which had been the property of Messrs. T. and W. Smith, and there the construction of marine boilers was undertaken, in addition to the existing manufacture of locomotives. In 1882 the works were enlarged to meet the growing business with many foreign navies, and in 1886 an amalgamation was effected with the shipbuilding firm of Andrew Leslie and Co., the name of the combined businesses becoming R. and W. Hawthorn, Leslie and Co., of which Mr. Browne was elected chairman. Thereafter the building of torpedo-boats and destroyers was added to the firm's activities, and the success which it achieved was very largely due to him. He was created a magistrate in 1877, and two years later was elected a member of the Newcastle City Council. He was Mayor of the city from 1885 to 1887, and in the latter year received the honour of knighthood. He studied economic questions very deeply, and served in 1905 on a special committee of the Home Office, which was appointed to consider the operation of the Workmen's Compensation Act. In 1890, when serious difficulties arose between the engineering and shipbuilding workers and their employers, he took a large part, under the lead of the late Colonel Dyer, in the organization which became the Engineering Employers' Federation. He was much interested in educational institutions, and took an active share in the founding of what is now the Armstrong College at Newcastle, and for his work in this connexion, the Durham University conferred upon him the honorary degree of D.C.L. He was Deputy-Lieutenant for the County of Northumberland, and a Justice of the Peace for the City of Newcastle and for Gloucestershire. He was a Member of this Institution from 1869 to 1884, and from 1902 until his death, which took place at Newcastle-on-Tyne on 1st March 1917, at the age of seventy-seven. In 1880 he read a Paper before this Institution on "Browne's Tramway Locomotive." He was also a Member of the

Institution of Civil Engineers, and a Member of the Institution of Naval Architects.

Engineer Vice-Admiral Sir Albert John Durston, K.C.B., was born at Devonport on 25th October 1846. At the age of fifteen he obtained, in open competition, an engineer studentship in Portsmouth Dockyard, where he served for over five years, and was entered as second-class assistant engineer in 1866. In the previous year he had gained a studentship in the Royal School of Naval Architecture in South Kensington, and had as contemporaries, Sir William H. White, Dr. Elgar, and Richard Sennett. His first commission was in H.M.S. "Ocean," on the China Station, where he continued for three years. Shortly after his return to England he was appointed, in October 1872, to Portsmouth, and five years later he was promoted to chief engineer. In 1881 he was transferred to Sheerness Dockyard as head of the engineering department, and two years later he returned to Portsmouth as chief engineering manager. In February 1886 he was promoted to Fleet Engineer, and Inspector of Machinery in 1888. A month earlier he had been appointed for service at the Admiralty, on the staff of the late Mr. Richard Sennett, and within five months he succeeded Mr. Sennett as Engineer-of-the-Fleet on his retirement. In 1893 he was promoted to be Chief Inspector of Machinery, which title was altered in 1903 to Engineer Rear-Admiral, and in 1906 he became Engineer Vice-Admiral. At the time of his assumption of the office of Engineer-of-the Fleet, steam pressures over 150 lb. in naval ships were rare, and the tendency was to subject the boilers to a high degree of forced draught to get the full power, and leaky tubes became a source of trouble. Sir John Durston introduced the French type of Belleville boiler, practically doubling the steam pressure. Subsequently other types were adopted, and the Belleville boiler was dispensed with. In 1901 the Parsons steam-turbine was introduced, first in destroyers and in 1905 in battleships—in H.M.S. "Dreadnought." In 1907 he retired from the service, having received a year's extension, and the Admiralty formally recorded their high appreciation of his

services. Official recognition was given of his work by his being made a Companion of the Order of the Bath in 1895 and a Knight Commander in 1897. His death took place at Blackheath, London, on 18th April 1917, in his seventy-first year. He was elected a Member of this Institution in 1899; he was also a Member of the Institution of Civil Engineers, an Honorary Vice-President of the Institution of Naval Architects, and had been President of the Institute of Marine Engineers.

ANDREW MORTON FYFFE was born at Dundee on 22nd November 1881. His professional training was received at St. Andrews University and the Dundee Technical Institute, and his apprenticeship was served under Mr. William Mackison, City Engineer of Dundee. For some time he was deputy engineer and manager of the City of Dundee Gas Department, and then at the age of twenty-eight he was appointed gas engineer and manager to the Nelson Corporation. During his term of five years the consumption of gas greatly increased, and the profits of the concern more than doubled. In March 1915 he left Nelson for Blackburn, when he was appointed to the post of gas engineer and manager. Just before his appointment the Blackburn Corporation had decided upon a reorganization of their gas undertaking, and Mr. Fyffe was recommended as an expert. Temporary measures were adopted to enable the department to carry on and to give him time to prepare his scheme. Unfortunately the Local Government Board withheld their sanction because the Treasury declined to give the necessary borrowing powers, and, excepting for the installation of a new tar distillation plant, the scheme had to be postponed till the end of the war. His death took place at Blackburn, after a few weeks' illness, on 15th January 1917, at the age of thirty-five. He became a Graduate of this Institution in 1904, an Associate Member in 1907, and a Member in 1912.

Sec. Lieut. GILBERT SUDBURY HALL, R.F.C., was born at Matlock on 28th December 1890, and was educated at Mill Hill

School. In October 1908, he began an apprenticeship in the engineering works of J. Carter Sons and Co., Ltd., Salford, and during his apprenticeship he took a course at the Manchester School of Technology. In November 1915, he joined the Royal Flying Corps, and after training went to the Front in May 1916. While on patrol work, his machine was shot down, the observer being killed at once. Lieut. Hall died of his wounds ten days later, 30th November 1916, in the enemy hospital at Cambrai, in his twenty-sixth year. He was elected a Graduate of this Institution in 1910.

John Percy Hall was born at Walker-on-Tyne on 10th December 1846. He served his apprenticeship on the Tyne, after which he went into the drawing office of the Wallsend Slipway and Engineering Co., Ltd., rapidly becoming chief draughtsman and shortly afterwards, about 1876, manager. In 1881 he left Wallsend to take charge of the Engine Works of Palmer's Shipbuilding and Iron Co., Ltd., where he was largely concerned in the development of the triple-expansion engine. Eight years later he left Jarrow to take up the post of managing director of John Penn and Sons, Ltd., Greenwich. While at Jarrow and Greenwich he had the control of the design and construction of many war-vessels, ranging from battleships to destroyers and scouts. In 1899 he founded the firm of J. P. Hall and Sons, Ltd., at Peterborough, for the construction of direct-acting pumps of his own design, which have become very well-known. His death took place at his residence at Sydenham, London, on 18th January 1917, at the age of seventy. He became a Member of this Institution in 1881; he was also one of the first Members of the North-East Coast Institution of Engineers and Shipbuilders.

Lieut. REGINALD HENRY BIRBECK HANDCOCK, A.O.D., was born at Stoke-on-Trent on 10th January 1880, and was educated at schools at Stoke-on-Trent, and at Newcastle-under-Lyme. He began his apprenticeship in 1899 in the works of the North Staffordshire Railway Co., Stoke-on-Trent, and completed it with

Messrs. James Simpson and Co., Ltd., Engineers, London. In October 1902 he became Assistant Engineer at Messrs. Drake and Gorham, Ltd., Electrical and Mechanical Engineers, Manchester, and in 1904 he was appointed chief draughtsman with Messrs. P. Willis, Sons and Co., Engineers, of Kingston-on-Thames. From 1906, he was with the Western Electric Co., North Woolwich, as Assistant Engineer, and was personally responsible for the design and supervision of electric plant on various installations until he joined H.M. Forces. He received his Commission as First Lieut., in the Army Ordnance Department in November 1915, and was appointed an Inspector of Ordnance Machinery. He left England for France in April 1916, and was in charge of a mobile light workshop until his death. He was drowned in the Somme on the 17th February 1917, at the age of thirty-seven. He was elected an Associate Member of this Institution in 1910.

Lieut. **WILLIAM CLAYTON HARVEY, King's Royal Rifles,** was born at Ventnor, Isle of Wight, on 9th December 1879. His early education having been received at schools in Hobart, Tasmania, at the age of eighteen he returned to England to study at the Crystal Palace School of Engineering, after which he attended classes at King's College, London. In 1902 he began his engineering training under Mr. W. G. Walker, of Westminster, and in the following year became a draughtsman in the London Office of Messrs. Dorman, Long and Co. Six years later he was appointed consulting engineer in London to the Government of Tasmania, and was holding this post up to the time of his receiving a Commission in the King's Royal Rifles. He was killed in action in France on 14th July 1916, in his thirty-seventh year. He was elected an Associate Member of this Institution in 1911.

Edward Hayes was born at Stony Stratford, Bucks, on 4th May 1845. He entered the engineering profession in 1863, as a pupil, under his father, at the Watling Works, Stony Stratford, where he remained for about five years. Subsequently he was engaged for about three years on the construction of small

steam-vessels and machinery on the River Weaver, being afterwards employed by the late Mr. Thomas Graham, of Leamington, on canal survey and experiments with light-draught vessels on canals and rivers. He took part in the management of the Watling Works from 1869 to 1876, during which time he was engaged on the construction of marine-engines and small steam-vessels, and on the death of his father the works became his property. Numbers of these vessels, together with stern-wheelers and paddle-boats, have been sent abroad, and he took honours at the Paris Exhibition in 1889 for his design of steamers for the London Metropolitan Fire Brigade. Examples of his work are afloat in various quarters of the world, for he had built steamers for the Admiralty, Crown Colonies, the Shah of Persia, the Sultan of Morocco, Trinity House, &c. He brought out numerous devices for improving steamers, including one for circulating water in an internal condenser by the action of the propeller, and more recently one for cheapening and facilitating the exportation of small steamers, making it possible to erect them at the site of their work. Many of his boats are engaged in the War, some of which are used for hunting submarines. His death took place at Stony Stratford, on 5th April 1917, in his seventy-second year. He was elected a Member of this Institution in 1882. He was also a Member of the Institution of Civil Engineers.

William King was born in Liverpool on 2nd June 1839, being the son of Alfred King, engineer of the Liverpool United Gas Light Co., and grandson of Joseph King, one of the original directors of the Liverpool Gas Light Co. He was educated at the Royal Institution School, Liverpool, and followed his father's profession, succeeding him on his death in 1867 as engineer of the Liverpool United Gas Light Co. During his term of office the company's operations were widely and rapidly developed. The extensive works at Bootle were established under his rule, and the Garston Works, entirely for water-gas, were also under his supervision. He retired from active work in 1904, and then

travelled extensively. His death took place at Waterloo, Liverpool, on 15th December 1916, at the age of seventy-seven. He became a Member of this Institution in 1872; he was also a Member of the Institution of Gas Engineers, and a Justice of the Peace for Liverpool.

Henry Alexander Longhurst was born in London on 30th July 1866. After receiving a scholastic education at Tunbridge Wells, he was apprenticed in January 1884 to the printing trade, and continued in this work until 1895 when he was employed by the International Typograph Co., in Berlin, where he afterwards entered the works of Messrs. Ludwig Loewe and Co. In 1898 he came to London and entered the service of the Linotype Co., Ltd., where he worked for two years in the experiment department, and then visited the Continent on their behalf to inspect and report on type-composing machines. In 1907 he left the Linotype Co. to join the the Typograph Co. of Berlin, as technical representative in the United Kingdom, and this post he held until November 1916 when he joined the Trench Warfare Supply Department of the Ministry of Munitions. His death took place in London on 21st February 1917, at the age of fifty. He was elected an Associate Member of this Institution in 1909.

Charles George Major was born at Basingstoke on 2nd May 1851. After attending the British School in his native town, he was apprenticed for six years to Mr. J. B. Soper, general engineer, of Basingstoke, whose firm later changed its name to P. and W. Hobbs, Basingstoke Iron Works. In 1870 he went as draughtsman to Henley's Telegraph Works, North Woolwich, and two years later became leading draughtsman, acting as assistant manager of the wire-rolling mill department. In this connexion he superintended the erection of rolling-mill and Siemens-Martin steel works, and the construction of cable-laying plant for several cable ships. In 1874 he went to Messrs. R. Warner and Co., hydraulic engineers, of Walton-on-the-Naze, to organize their first drawing office, and in 1876 was appointed works manager.

In the following year he became manager for Messrs. Withinshaw and Co., rolling mill and colliery engineers, of Birmingham, and returned to London in 1878, when he took up the post of manager to Messrs. Archibald Smith and Co., hydraulic and general engineers, of Leicester Square, London. Two years later a new factory was built at Battersea to meet the expansion of the business, and the firm's name was altered to Archibald Smith and Stevens. In 1889 he became a partner, and was senior partner from 1904. In 1909, owing to further expansion of the business, it was decided to convert it into a private limited company under the name of Smith, Major and Stevens, Ltd., with Mr. Major as chairman, and to remove the works to Northampton, where works and offices were built to his plans. From the time he joined the firm he devoted his energies entirely to the development of lift and hoist design and manufacture, both hydraulic and electric, and he was the inventor of a great number of improvements in this class of machinery. In the early days of hydraulic lifts he invented a form of the balance cylinder with which the name of the late Mr. Ellington is more generally associated. Both men were working on the problem and arrived at almost identical solutions independently. He also devoted much time to the evolution of safety devices for lifts; and developed a large number of types of electric locks for lift-well doors. His death took place at Leamington after a short illness, on 15th February 1917, in his sixty-sixth year. He was elected a Member of this Institution in 1906.

Sec. Lieut. **BERESFORD FRANK PARSONS, R.F.C.**, was born in Birmingham on 18th March 1892. He received his early education at Solihull Grammar School and at King Edward's High School, Birmingham. In 1908 he began an apprenticeship of four years at the Birmingham Corporation Gas Works, and during the same period studied at the Birmingham Technical School. On the completion of his term he remained at the Gasworks as assistant engineer. In 1914 he joined the 6th Warwicks Territorials, and in the following March went to France. After 13 or 14 months he

came home for his Commission, which he obtained in October 1916, and was attached to the Royal Flying Corps. His death took place from an accident while flying, on 23rd January 1917, in his twenty-fifth year. He became a Graduate of this Institution in 1913.

Herbert Pilkington was born at Pilkington, near Radcliffe, Manchester, on 23rd October 1864. He received his education at Owens College, Manchester, and then became furnace manager of the Outwood Coal and Iron Co., Ltd., near Manchester. From there he went to Messrs. W. Roberts and Co., Tipton, as works manager, and subsequently to the Midland Coal and Iron Co.'s Works at Newcastle, Staffordshire. After a further term with the Wellingborough Iron Co., Ltd., he received the appointment of works manager under the Sheepbridge Coal and Iron Co., Ltd., Chesterfield, with whom he retained that position until his death, a period of nineteen years. In local affairs he was for many years a member of the Whittington and Newbold Urban District Council, of which he was Chairman at the time of his death, which took place at Chesterfield on 24th November 1916, at the age of fifty-two. He became a Graduate of this Institution in 1888, and a Member in 1890. He had been President of the South Staffordshire Iron and Steel Institute for two years, and President of the British Foundrymen's Association.

Lieut. HAROLD BRINDLEY ROBB, A.S.C. (M.T.), was born in London on 25th March 1893. He received his early education at schools in Balham and Clapham and at the Battersea Polytechnic, after which he took a three-years' course at the City and Guilds Technical College, Finsbury, gaining diplomas in civil and mechanical engineering. On the completion of his College course he entered the firm of Messrs. H. Young and Co., constructional engineers, of Nine Elms, where he was engaged on the design of various kinds of steel structures, until the outbreak of the War, when he joined the London University O.T.C., from which he was commissioned to the Royal Fusiliers, and was later transferred to

the Motor Transport Section of the Army Service Corps. In this position he was engaged in designing and erecting several small buildings for his Company at the Front, and in the construction of roads. Later he became an anti-gas instructor and Officer in charge of Section for the felling of trees for the supply of wood and fuel. He was mentioned in Dispatches on 5th January 1917 by Sir Douglas Haig for distinguished service during the fighting on the Somme. His death took place at the Millbank Hospital from illness contracted abroad, on 2nd January 1917, in his twenty-fourth year. He became a Graduate of this Institution in 1912.

Engineer Lieut.-Commander **ARCHIBALD SAMUEL DE ST. LEGIER, R.N.**, was born at Loughborough on 6th November 1882. He was educated at Loughborough Grammar School and at St. Paul's School, West Kensington. Having taken a special course of instruction at Portsmouth, he passed 8th out of 200 candidates in the examination for Cadets for the Royal Naval Engineering College at Devonport, which he entered in 1899. On the completion there of his five years' term in 1904, he became Engineer Sub-Lieutenant on H.M.S. "Majestic," and afterwards served on the "Aboukir," "Bedford," "Warrior," and "Falmouth." In 1907 he was promoted to Engineer Lieutenant, and in 1915 to Engineer Lieut.-Commander. He was on H.M.S. "Bedford" when she was wrecked on the coast of Japan, and on H.M.S. "Falmouth" at the Battle of Heligoland, when she sank the German Cruiser "Mainz." His last appointment was to the Destroyer "Negro," which was commissioned in May 1916. This ship was run into and sunk by another Destroyer on 21st December 1916, all the officers and 49 men being lost. He was thus thirty-four years of age. He was elected an Associate Member of this Institution in 1907.

Robert Gordon Sharp was born at Keith, Banffshire, on 19th March 1864. He was educated at Gordon's College, Aberdeen, and the Yorkshire College, Leeds. His apprenticeship was served in the locomotive shops of the Great North of Scotland Railway

Co., at Aberdeen, from 1882 to 1887, when he went into the drawing-office for a year. Later he went into the drawing-office at Kitson and Co.'s Airedale Foundry, Leeds. Subsequently he held appointments in the locomotive departments of the Great North of Scotland Railway at Aberdeen and of the Midland Railway at Derby. In 1904 he was appointed assistant works manager to the Leeds Forge Co., and in 1911, when Mr. F. L. Lane became managing director, succeeded him as general manager. His death took place at Chapel Allerton, Leeds, on 21st March 1917, at the age of fifty-three. He was elected a Member of this Institution in 1905.

George Percival Spooner was born at Beddgelert, Carnarvon, on 13th June 1850. After attending a local school he went to Harrow School, and later to the Polytechnic at Karlsruhe. He was articled to his father, Mr. C. E. Spooner, manager and engineer of the Festiniog Railway, North Wales, and passed through the shops and drawing office. During this period, and subsequently, he was engaged on surveying, levelling, draining, and the design and construction of reservoirs and dams, of locomotives, rolling stock, stations and general railway equipment. In 1879 he went to India as assistant district and locomotive superintendent on the Indian State Railways, and on his retirement in 1894 he returned to England where he interested himself in scientific instruments and organ building. In the early part of the War he became a Special Constable with the rank of Sergeant. His death took place in London after a short illness on 21st January 1917, in his sixty-seventh year. He was elected a Member of this Institution in 1885.

Private **HUGH GORDON STEPHENS, 16th Middlesex Regiment,** was born in Madras on 8th May 1894. His scholastic education was received in Bangalore, S. India, and at the age of nineteen he went to England to commence a pupilage in the locomotive works of the North Staffordshire Railway. On the completion of his pupilage, he was preparing to enter the Engineering

Faculty of University College, London, but gave up all the chances of a University and professional career, and joined the 16th Middlesex Regiment in June 1915. He went to France in September 1915, and took his part in the fighting on the Somme on 1st July 1916. He was killed at Ypres, on the 12th September 1916, at the age of twenty-two. He became a Graduate of this Institution in 1914.

James Stirling was born at Galston, Ayrshire, on 2nd October 1835. He was educated privately and at the age of eighteen was sent to the village millwright, two years later commencing his apprenticeship under his brother, Patrick, then locomotive superintendent of the Glasgow and South Western Railway Co., Glasgow. In the following year he was sent to Kilmarnock and passed through all the locomotive departments. On the completion of his apprenticeship he was made a charge-man, and subsequently worked in the drawing-office. With a view to varying his experience, he left Kilmarnock and went to Manchester, where he entered the service of Messrs. Sharp, Stewart and Co., as a working fitter. A year later he returned to Kilmarnock and re-entered the drawing-office, subsequently becoming works manager, which position he held until 1866. On his brother, Patrick, leaving to take up the position of locomotive superintendent of the Great Northern Railway, the directors of the Glasgow and South Western Railway appointed him as successor to his brother, which position he occupied until 1878. During his service as locomotive, carriage and wagon superintendent, he introduced a more powerful class of passenger engines, with 6 feet 6-inch coupled wheels and 18-inch by 24-inch cylinders. When the proposal to run trains in direct connexion with the Midland Railway came up, he built a still more powerful class of engine with 7 feet coupled wheels, a leading bogie and inside cylinders, 18 inches by 26 inches, with the valves between. This type of engine was for many years the standard design for express passenger work in Great Britain. In 1874 he introduced a steam reversing-gear for locomotives, and he was the first

engineer to provide cushioned seats for third-class compartments. In 1878 he accepted an offer to become locomotive superintendent of the South Eastern Railway, and held this position until his retirement in 1898. During that period many improvements in the locomotive power on the railway were introduced by him, among which may be mentioned a special type of bogie, and the steam reversing-gear which had been so successful on the Glasgow and South Western Railway. He was the first engineer to use 19-inch cylinders with the valves between them. When in Kilmarnock he was Hon. Secretary to the Scottish Locomotive Railway Engineers' Association, and when he removed to England, on the formation of the Association of Locomotive Engineers of Great Britain and Ireland, he was appointed Hon. Secretary, a position which he held until his retirement from the South Eastern Railway. His death took place at Ashford, Kent, on 12th January 1917, at the age of eighty-one. He became a Member of this Institution in 1880; he was also a Member of the Institution of Civil Engineers, and a Justice of the Peace for Kent.

Richard Beaumont Thomas was born at Oxford on 25th May 1860, being the eldest surviving son of the late Mr. Richard Thomas, of Lydbrook. On completing his education at Clifton College, and being destined by his father for a business career, he spent some years in the great manufacturing centres in the north of England, and then went for a short time to the Ebbw Vale Steel Works. This was followed by his joining the firm of which his father was the founder, namely, Richard Thomas and Co., Ltd., Lydney, to which was subsequently added the tin-plate works at Llanelly, Swansea, and elsewhere, thus making the firm the supreme one in the tin-plate industry in Wales. He was keenly interested in elementary and secondary education, and aided with generous contributions the Technical College at Llanelly, the University College at Cardiff, and the Swansea Technical College. On the outbreak of the War, he realized the magnitude of the contest, and on the demand for more munitions he quickly placed all the resources of his Company at the disposal of the Government. He

also busied himself in schemes for inducing his workmen to subscribe to the War Loan. His death took place at his residence at Englefield Green, Surrey, on 14th February 1917, in his fifty-seventh year. He was elected a Member of this Institution at the Cardiff Meeting in 1906, when he presented a Paper on "The Manufacture of Tin-Plates." He was a Justice of the Peace for the County of Gloucester.

Sec. Lieut. **WILLIAM CREW TREMEARNE,** Seaforth Highlanders, was born at Leamington Spa on 13th January 1886. He was educated at Blackheath School, and at the age of seventeen began his apprenticeship at the works of Dick, Kerr and Co., Kilmarnock. In 1904 he went to Preston and was engaged in erecting electrical machinery for the same firm, subsequently acting as their assistant engineer on tramway construction at Rochester, Borstal, Maidstone, and London. From 1910 to 1913 he studied at Christ's College, Cambridge, taking the Mechanical Sciences Course, and later on his B.A., and in the latter year he received the appointment of assistant engineer to the Singapore Tramway Co., having charge of the power-house, supervision of the boiler plant and outside track and overhead system. In 1914 he returned from Singapore, invalided with fever, just before the outbreak of the War, and rejoined the squadron he had previously joined when at Cambridge, namely, King Edward's Horse. Later he obtained a Commission in the Seaforth Highlanders, with whom he went to the Front, and was machine-gun officer to his battalion, He was severely wounded at Loos and again at Hill 70, and was reported missing. In April 1917 he was presumed to have been killed on 25th September 1915, in his thirtieth year. He was elected an Associate Member of this Institution in 1913.

Private **LESLIE HENRY WORSSAM, H.A.C.,** was born in London, on 3rd February 1885. He was educated at Aldenham School, and University College, London, and subsequently served a five years' apprenticeship from 1902 to 1907 with his father's firm, Messrs. G. J. Worssam and Son, brewery engineers and

coppersmiths, London. On its completion he entered the drawing-office and took charge of outdoor work, subsequently becoming head draughtsman. On the conversion of the firm into a limited liability company in 1912, he became one of the directors under his father's chairmanship. He went to France with the Hon. Artillery Company in December 1914, was wounded at Hooge on 16th June 1915, and killed in action at Hamel on 8th February 1917, at the age of thirty-two. He became a Graduate of this Institution in 1903, and an Associate Member in 1911.

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(Mr. E. M. Eden's remarks)

Micrometers for Measuring Screws.

Fig. 8. *Sliding Machine.*

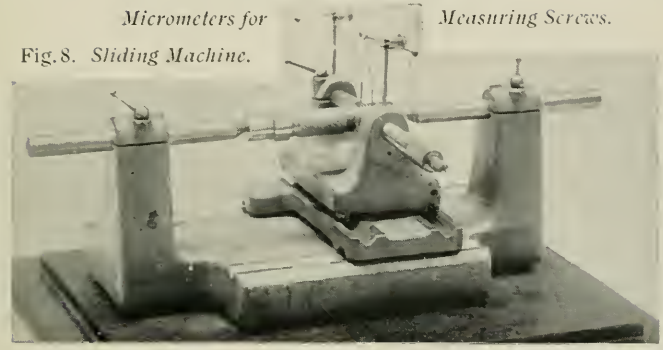
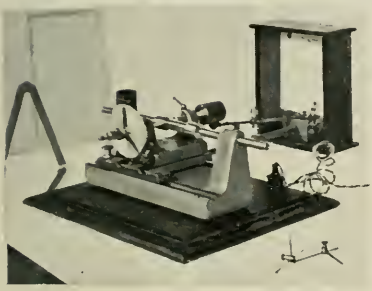


Fig. 9. *Optical.*

Fig. 11. *For Workshop use.*



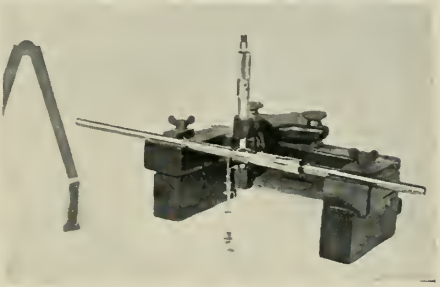
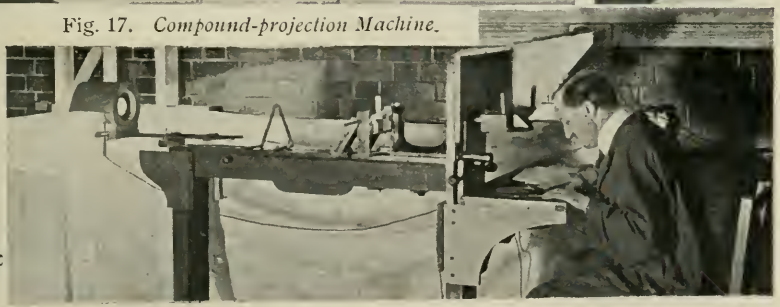


Fig. 13. *For Screws up to 6 inches.*

Fig. 14.

Photo of Screw, 36 per inch.

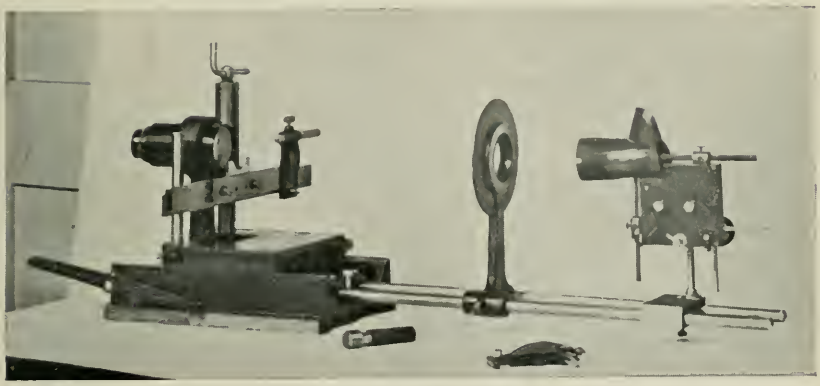
Fig. 17. *Compound-projection Machine.*



Mechanical Engineers 1917.

(Mr. E. M. Eden's remarks, continued.)

Fig. 18. *Projection Machine with large field of view.*



(Mr. P. A. Bentley's remarks.)

Lapping Machines.

Fig. 21.
Fitted on Lathe Mounting.

Fig. 23.
For Flat-surface work, with micrometer adjustment.

Fig. 22. *Fitted for ordinary Workshop use.*

Fig. 5. *Slow-Speed Alternating Stress Machine.*

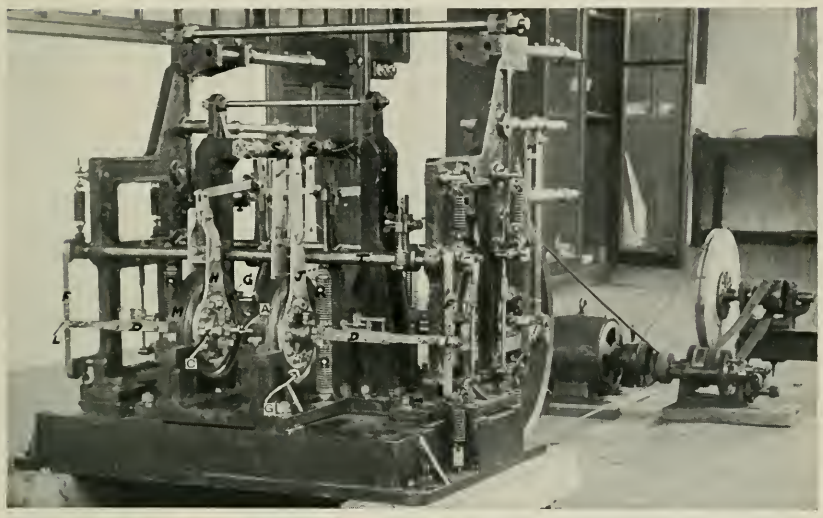


Fig. 15. *Specimens of Alternating Torsion Fractures. (See Tables 2, 3 & 5.)*

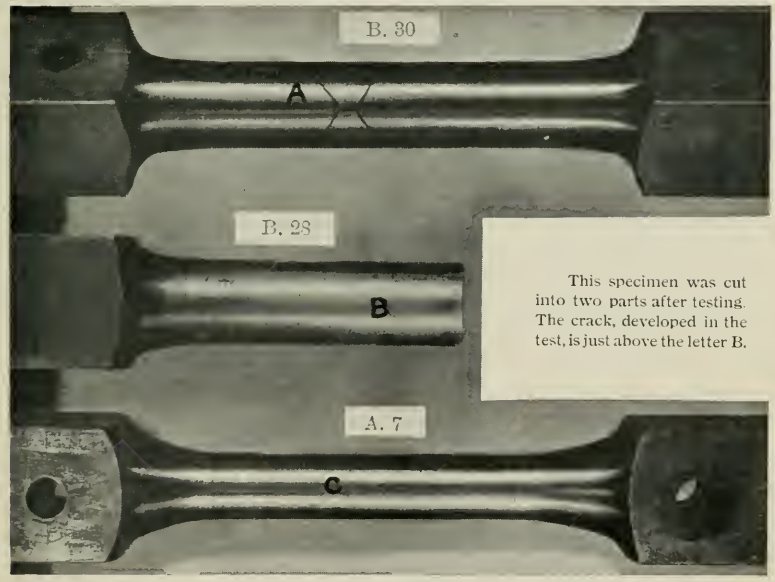

B. 30
A
B. 28
B
This specimen was cut into two parts after testing. The crack, developed in the test, is just above the letter B.
A. 7
C

Fig. 16. *(See Tables 3, 4 & 5.)*
Alternating Torsion Fracture at D. Alternating Bending Fracture at E.

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Fig. 5. *Tensile and Bending Tests with Photomicrographs.* × *100 d.*

		Tensile	Elong'n	Redu.	Bend.
A.	*Forged and Oil Treated*	*51·5*	*3·2*	*5·7*	*10°*
B.	*After correct Heat Treatment*	*50·6*	*16·6*	*24·1*	*180°*

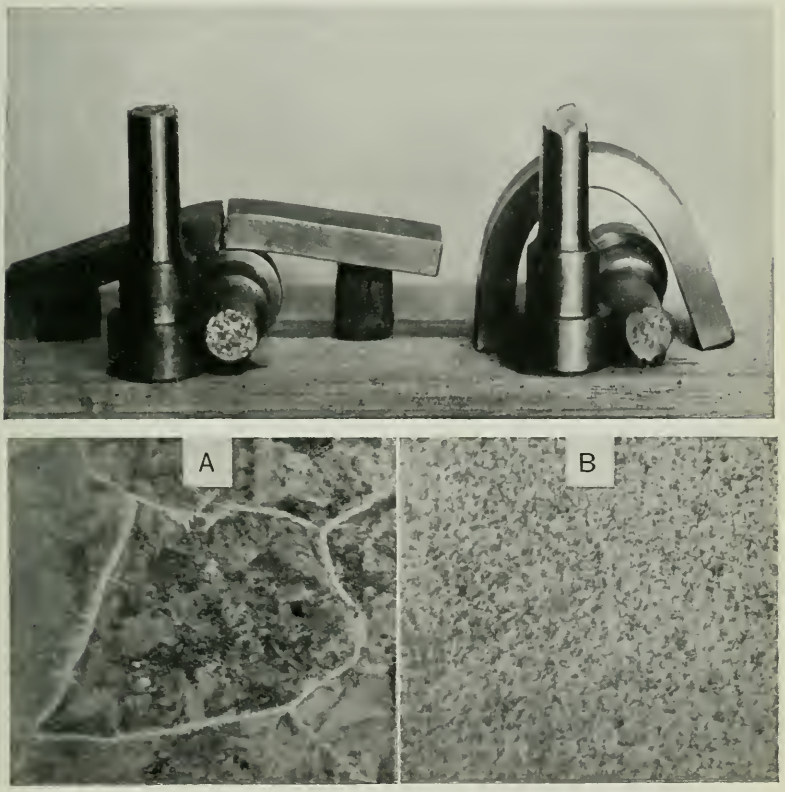


Fig. 6. *Miscellaneous Forgings in the Furnace. Illustrating use of Pyrometers.*

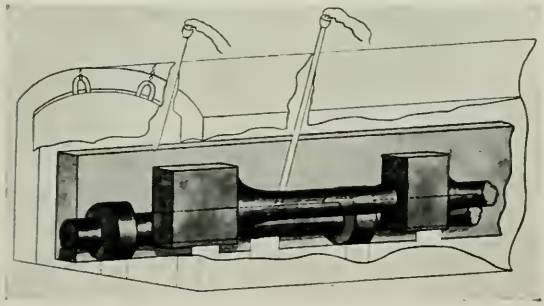
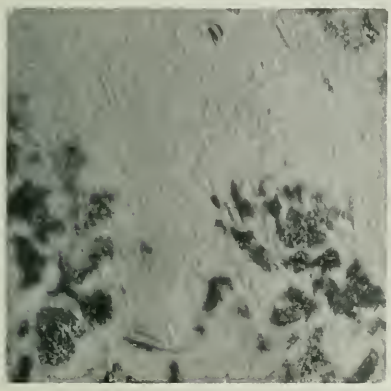


Fig. 7. *Heavy Steel Forgings.* ×100 *diams.*

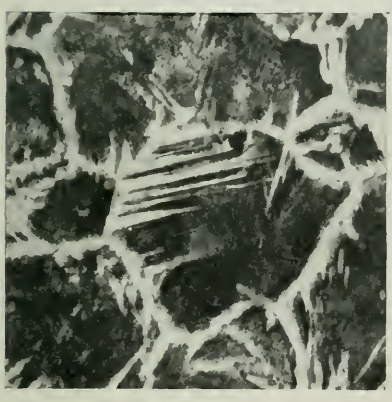
Low-Carbon Steel.

Lightly Annealed. *Drastically Overheated.*



Medium-Carbon Steel.

Drastically Overheated. *Well Annealed.*



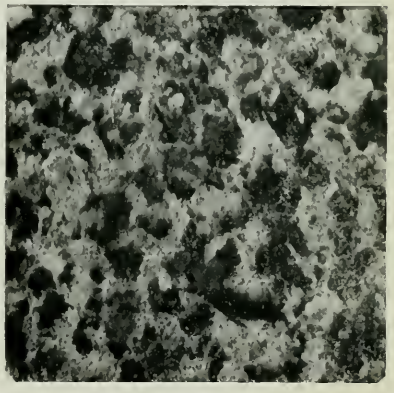


Fig. 8. *Bad and Good Annealing of Low-Carbon Steel.* × 25 *d.*

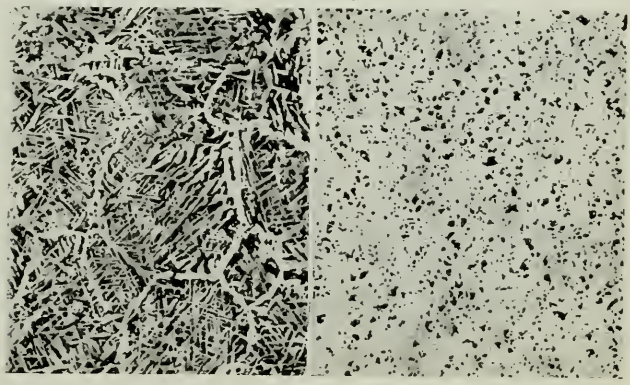


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Fig. 9. *Diagram showing Forging operations on Marine Shaft.*

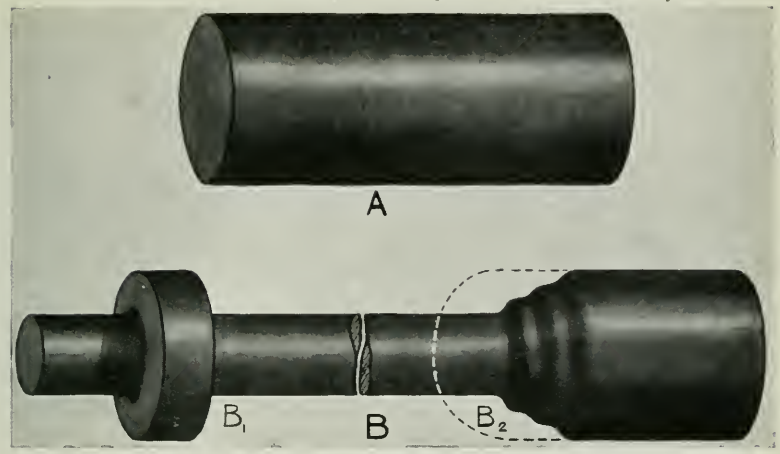


Fig. 10. *Unannealed Steel Casting.*
× 25 *diams.*

Fig. 11. *Heavy Bolt broken under Shock Test.*

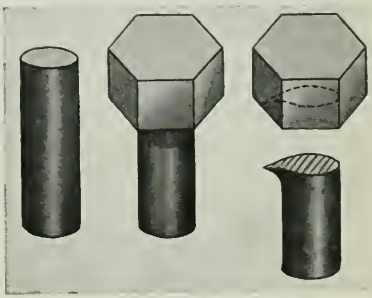
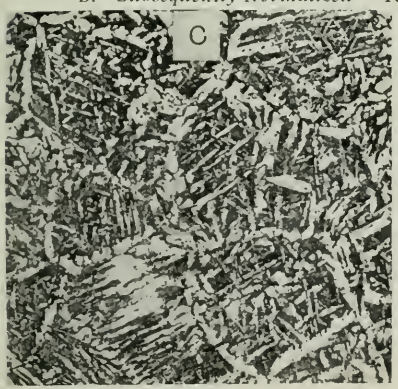


Fig. 12. × *100 d.*

	Yield	Tensile	Elong'n	Redu.	Bend.
C. *Drastically Overheated*	*16·8*	*31·5*	*16·5*	*24·0*	*70°*
D. *Subsequently Normalized*	*18·5*	*32·8*	*29·8*	*52·3*	*180°*



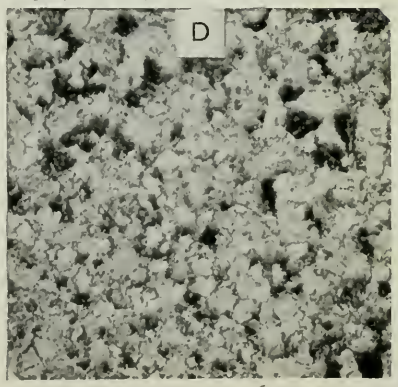
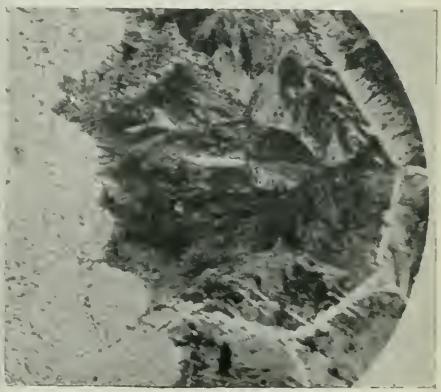


Fig. 13.

Etched piece of Steel.

Actual size. *Photomicrograph on junction line. × 100 diams.*



Figs. 14 and 15. *Furnace diagrams illustrating use of Pyrometers.*

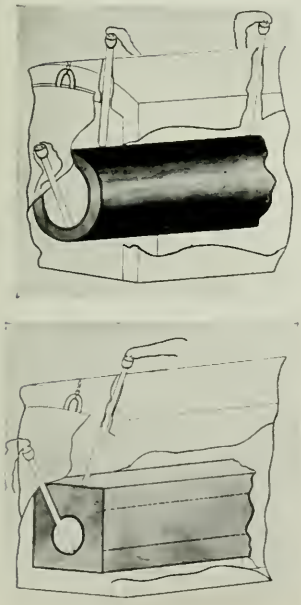


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Fig. 16. *Furnace diagram illustrating use of Pyrometers.*

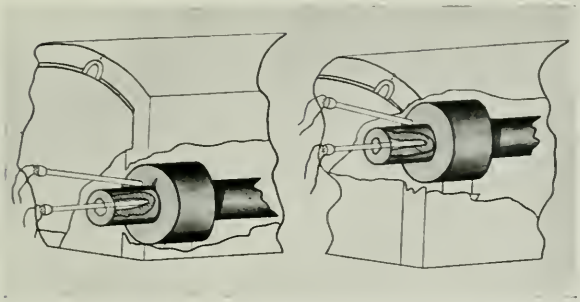


Fig. 17. *One of the uses of the Brearley Sentinel Pyrometer.*

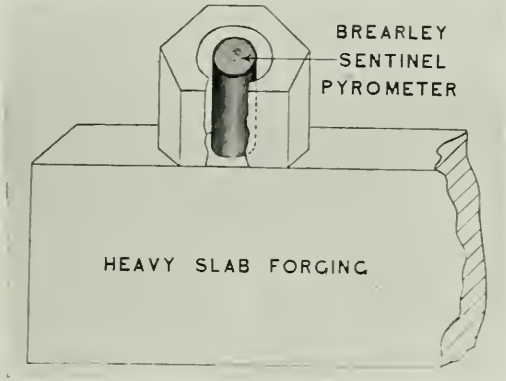
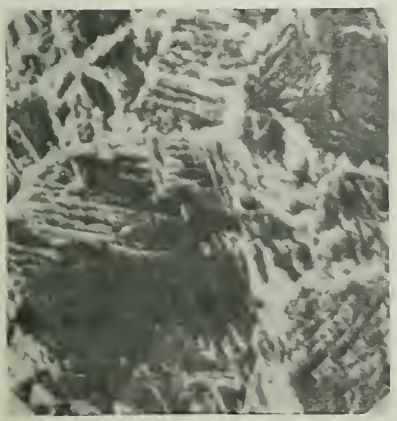


Fig. 18. *30-Ton Forging.* × *100 diams.*
Effect of Quenching and Tempering.

Drastically overheated before quenching.



Refined and Normally heated before quenching.

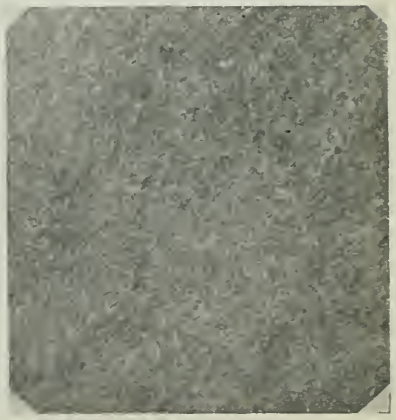


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Fig. 20.

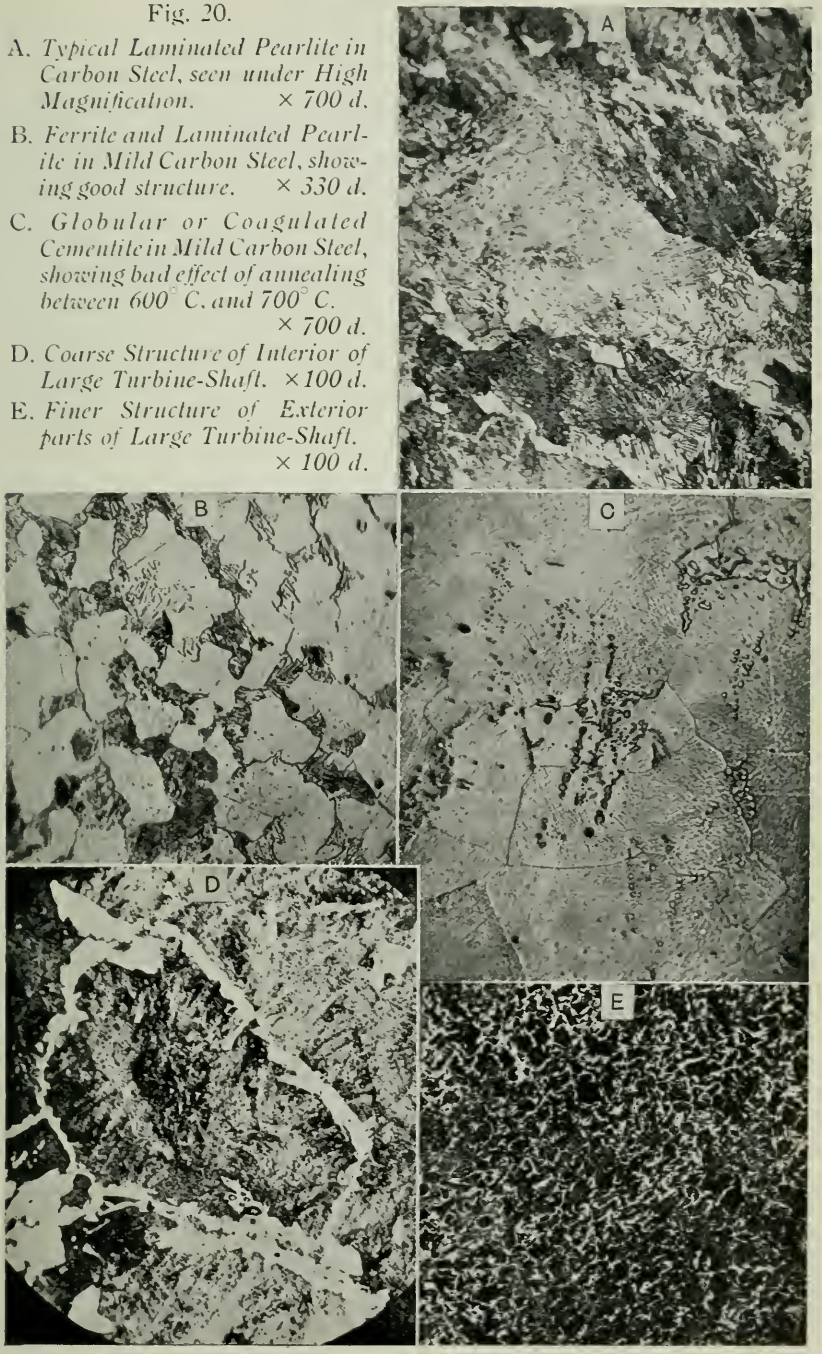
A. *Typical Laminated Pearlite in Carbon Steel, seen under High Magnification.* × 700 *d.*

B. *Ferrite and Laminated Pearlite in Mild Carbon Steel, showing good structure.* × 330 *d.*

C. *Globular or Coagulated Cementite in Mild Carbon Steel, showing bad effect of annealing between 600° C. and 700° C.* × 700 *d.*

D. *Coarse Structure of Interior of Large Turbine-Shaft.* ×100 *d.*

E. *Finer Structure of Exterior parts of Large Turbine-Shaft.* × 100 *d.*



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